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Proceedings at the Meetings, and Report of the President and Council for the Session 1889–90.

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PROCEEDINGS

OF

THE PHYSICAL SOCIETY

OF LONDON.

OCTOBER 1888.

I. *Note on the Conditions of Self-Excitation in a Dynamo Machine.* By SILVANUS P. THOMPSON, *D.Sc.*[*]

WHEN about the year 1867 it began to be recognized that a machine constructed on the principle of magneto-electric induction might be made to excite the magnetism in its own field-magnets, it became usual to explain the phenomenon of self-excitation in language something like the following. There being a small amount of residual magnetism in the core of the stationary electromagnet, very feeble currents will be induced in the secondary or armature-coils when they are set into rotation at any given speed. If these feeble induced currents are now sent round the coil of the fixed electromagnet they will exalt its feeble magnetism, and its inductive action will be stronger; these stronger currents being also sent round the magnet will raise its magnetism still higher, and so by action and reaction the magnetism and the currents grow at a compound-interest rate until the limit fixed by the magnetic saturation of the iron core of the field-magnet is reached.

Long before the true rationale of the limit of self-excitation was assignable, it was known that this method of considering the subject was faulty. Were it true, it would follow that, no matter what the speed of driving, or the resistance in the electric circuit, the magnetism would go on rising until the iron had attained saturation. The facts, on the contrary, are (1) that in self-exciting machines the degree of excitation of the field-magnets depends most emphatically on the speed and on the resistances of the electric circuit; (2) that for machines which have their exciting coil in series with the armature and main circuit, there is a certain relation between speed and resistance such that they do not excite themselves at all unless the quotient of speed by resistance attains a certain minimum value; (3) that in machines which have their exciting coil connected as a shunt to the main circuit there is again a certain relation between the speed and the electric resistances of the various parts of the circuit, such that with a given speed self-excitation does not take place unless the resistance of the external circuit exceeds a certain limiting value.

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[*] Read May 12, 1888.

On June 26, 1886, I had the honour of bringing before the Physical Society* some formulæ relating to the performance of dynamo-electric machines, in which this matter was touched upon, and in which, on the assumption that the law of magnetization might be adequately represented by the Lamont-Frölich formula, expressions were obtained for the relations between speed, magnetization, resistance, and current. As an important conclusion it was shown, always upon the above assumption, that at a given speed of driving and with a given resistance in the electric circuit, the current, of any given series-wound machine, was always less by a certain fixed quantity than that current which would have been produced (at that speed and with the given resistance) had the magnetism been actually at full saturation. This difference between the actual current and the current that there would have been at saturation was a definite number of amperes, and was shown to be that number of amperes which would have been sufficient to produce exactly half-saturation in the given machine. This half-saturating number of amperes I had previously denominated the "Diacritical" current. This proposition having been established, it became obvious that the machine would not excite itself if the speed and resistance were such—the speed so low or the resistance so high—that the current which would have been produced with field-magnets independently saturated was less than the diacritical current. Or, if the resistance of the circuit were prescribed, there would obviously be a least-speed of self-excitation.

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* See Proc. Phys. Soc. vol. viii. p. 76.

The assumption in the above investigation of an approximate expression for the law of magnetization necessarily made the argument less satisfactory than would be the case were no such assumption made. In the argument which follows no assumptions are made of an empirical character as to the law of magnetization, absence of secondary phenomena, such as electric and magnetic leakage, being the only point assumed.

It is known that the electromotive force of a dynamo may be written

$$E = nCN; \quad \ldots \ldots \quad (1)$$

where E is the electromotive force (in absolute C.G.S. units); *n* the number of revolutions of the armature per second; C the number of conductors counted round the periphery of the armature, and closed in series; N the whole number of magnetic lines (in C.G.S. units) that traverse the armature.

So, writing ΣR for the sum of the electric resistances (in C.G.S. units) of the circuit in which the electromotive force operates, the current flowing in the circuit may be written as

$$i = \frac{nCN}{\Sigma R}. \quad \ldots \ldots \quad (2)$$

Again, it is known that the number of magnetic lines in the armature may be calculated by dividing the line-integral of the magnetizing forces by the sum of the magnetic resistances of the "magnetic circuit" of the dynamo. If the machine is joined up with its exciting coil of S convolutions in series with the main circuit, the line-integral of the magnetizing forces will be written as $4\pi Si$. Then, writing $\Sigma\rho$ as the sum of all the magnetic resistances in the magnetic circuit, we shall have

$$N = \frac{4\pi Si}{\Sigma\rho}. \quad \ldots \ldots \quad (3)$$

From (2) we get

$$\frac{i}{N} = \frac{nC}{\Sigma R}; \quad \ldots \ldots \quad (4)$$

and, from (3),

$$\frac{i}{N}=\frac{\Sigma\rho}{4\pi S}. \qquad (5)$$

Equating (4) and (5), we have

$$4\pi S n C=\Sigma\rho \,.\, \Sigma R. \qquad (6)$$

It will be noticed that this equation contains neither N nor i, and its truth is independent of the relation between i and N. It is true that $\Sigma\rho$, which is a factor of the expression on the right-hand side, depends on N, and, in general, increases when N is increased. But the truth of the equation is not affected by the form of the relation between $\Sigma\rho$ and N ; in brief, it is true independently of any assumptions as to the form of the law of magnetization.

On examining the expression on the left hand of the equation, it will be seen that, for a given dynamo, all its factors, except the speed n, are invariable. Hence the main fact brought out by this equation is that in a series dynamo, driven at constant speed, the product of the magnetic resistance into the electric resistance is a constant. We have here the key to the behaviour of the self-exciting dynamo ;—the explanation why it is that the machine, though exciting itself up "at a compound-interest rate," stops short of absolute saturation in the degree to which its magnetization is raised. As the field-magnets get more and more highly magnetized, the magnetic resistance increases. Suppose that such a dynamo, running at a constant speed, works at a certain degree of magnetization when there is a certain resistance in the electric circuit. Suppose that resistance to be now reduced : the current increases ; as a result there is higher magnetization, higher electromotive force, and therefore still greater current. To what point will these things go on increasing? Equation (6) gives the answer: until the decrease in the electric resistance is exactly balanced by the increase in the magnetic resistance.

Other phenomena of the action of the dynamo now become more intelligible. As is well known, in ascending magnetizations of iron there is a certain stage at which the magnetic permeability increases, instead of decreasing, with an increase in the magnetization. In a dynamo at this stage of excitation, any increase in the electric resistance, causing a decrease in the current, will also cause an increase in the magnetic resistance; hence this stage of excitation is one of instability. The dynamo is either excited above this stage, or else its excitation falls below this stage—virtually to zero.

The two factors of the product $\Sigma\rho \,.\, \Sigma\mathrm{R}$ are both complex quantities, containing constant as well as variable terms. We may write

$$\Sigma\rho = \alpha + \xi.$$

Here the constant part α relates to the magnetic resistance of the gap between the armature-core and the polar surface of the field-magnets; which gap, filled partly with copper, partly with air, and partly with insulating materials, possesses, so far as is known, a constant magnetic resistance. The variable part ξ of the magnetic resistance relates to the iron portions of the magnetic circuit: it is in general the sum of a number of terms such as $l/\mu\mathrm{A}$, where l is the length, A the sectional area, and μ the permeability, for the time being, of the various portions—cores, yoke, &c.

Similarly, we may write

$$\Sigma\mathrm{R} = a + x;$$

where a stands for the constant part of the electric resistances (armature and main-circuit coils) in the circuit, and x for the variable part; that is to say, the resistance of the external circuit, including under that term not only true ohmic resistances, but also the effects of counter electromotive forces opposed in the path of the current of the machine.

We may now rewrite equation (6) in the following form for the series-wound machine driven at a constant speed:—

$$(\alpha + \xi)(a + x) = \text{constant}. \quad . \quad . \quad . \quad . \quad (7)$$

Now at starting, when as yet the magnetization of the iron is almost zero, the resistance of the iron part of the magnetic circuit is small. Its value is not indeed quite zero, but depends upon the prior history of the iron. It is certainly so small as to be negligible compared with the value of α. Hence there will be a particular maximum value of x, which we may call x_1, corresponding to the case when $\xi = 0$. This maximum value, which will be given by the equation

$$x_1 = \frac{4\pi n\mathrm{CS}}{\alpha} - a, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

will be the critical external resistance of the machine at the prescribed speed. If any greater resistance than x_1 be intercalated in the circuit the machine will refuse to excite itself, even though there is a small initial magnetization present. So far as there is a weak magnetization present, the machine will act as a feeble magneto-electric machine.

Suppose, now, that x is so far decreased that the total electric resistance is reduced to half its limiting value, that is to say, let $x'=\frac{1}{2}x_1-\frac{1}{2}a$: then the current and the magnetization will both run up until the magnetic resistance is equal to double its initial value, and ξ will have the value $\xi'=\alpha$. The permeability of the magnetic circuit, taken as a whole (iron, copper, and air), will have been halved. This state of things corresponds to the "diacritical point" in the excitation first investigated by the author in 1884; and, if the Lamont-Frölich formula were true, this point would correspond to the state of semi-saturation of the magnetization.

Again, suppose the variable part of the electric resistance to be reduced to zero; in other words, let the machine be short-circuited. Then ξ will run up to the maximum value it can have at that speed. Calling this value ξ_2, we have

$$\xi_2=\frac{4\pi n\mathrm{CS}}{a}-\alpha. \quad . \quad . \quad . \quad . \quad . \quad (9)$$

It is further clear that if a fixed value be given to the external electric resistance x, there will be a limit in speed below which the machine will refuse to excite itself, the least-speed of self-excitation (called by some authorities the "dead turns," by others the "critical velocity") being given by the equation

$$n_1=\frac{\alpha(a+x)}{4\pi\mathrm{CS}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

This throws some light on the observed fact that very small machines are not self-exciting except at very high speeds. The fixed part α of the magnetic resistance varies in similar machines of different size, inversely as the linear dimensions; also the fixed part a of the electric resistance varies inversely as the linear dimensions of the machine. Hence the least-speed of self-excitation in similar machines (on short-circuit) varies inversely as the square of the linear dimensions. In practice it is possible, in larger sizes of machine, to reduce both α and a to amounts relatively smaller; for the actual air-space left for clearance may be relatively smaller in large machines, and a relatively greater thickness of copper on the armature-periphery is admissible. When the machine is on short-circuit, $x=0$, and the last equation then becomes

$$n_1=\frac{\alpha a}{4\pi \mathrm{CS}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

This, too, is important as showing that the least-speed of self-excitation of a machine while short-circuited is a measure of the goodness of the magnetic circuit.

For a shunt-wound dynamo the fundamental equation becomes

$$(\alpha+\xi)\left(a+s+\frac{as}{x}\right)=4\pi n\mathrm{CZ}; \quad . \quad . \quad . \quad (12)$$

where a is, as before, the resistance of the armature-circuit within the machine, s the resistance of the exciting shunt-circuit, and Z the number of convolutions of the shunt-coil. Excitation does not take place at a given speed n unless x has as its *minimum* value

$$x=\frac{\alpha as}{4\pi n\mathrm{CZ}-\alpha(a+s)}. \quad . \quad . \quad . \quad . \quad (13)$$

On open circuit, when $x=\infty$, the least-speed of self-excitation is

$$n_1=\frac{\alpha(a+s)}{4\pi \mathrm{CZ}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

the machine then exciting itself through its own circuit as a series-machine. If short-circuited it will require an infinite speed to become self-exciting.

There is a still more general way of establishing the relation (6). Every dynamo may be regarded as an arrangement of two circuits, a primary (or field-magnet) circuit and a secondary (or induced or armature) circuit, between which there is a coefficient of mutual induction M, which is caused to vary by mechanically altering the configuration of the system. If the currents in the two circuits be called respectively i_1 and i_2, then the work done in starting or stopping either current in the presence of the other is equal to $\mathrm{M}i_1i_2$. Now in each revolution of the armature of an ordinary dynamo the current in each of the sections of the armature-coil is twice started and twice stopped. Hence in each semi-revolution the work to be mechanically performed in driving is equal to Mi_1i_2; or if, as in the series-dynamo, the current in the two circuits is the same, the work to be done is Mi^2. Now, on the assumption that there is no magnetic leakage, the value of M is equal to $4\pi S_1S_2 \div \Sigma\rho$; where S_1 and S_2 are the respective numbers of convolutions of the primary and secondary coils which surround the tubes of magnetic force in the magnetic circuit. For our purpose, S_2, in ordinary armatures, with symmetrically-wound coils united in a self-closed circuit, tapped at two opposite points by the two contact-brushes, and with commutation occurring at points where each commuted convolution encloses a maximum magnetic flux, is equal to $\frac{1}{2}C$. But the work done by the machine in the electric circuit during a semi-revolution is equal to $i^2\Sigma R \div 2n$. Equating this to the work done on the machine gives, as before,

$$\Sigma\rho \,.\, \Sigma R = 4\pi n CS.$$

City and Guilds Technical College,
Finsbury, June 1888.

II. *On a Simple Apparatus for the Measurement of the Coefficients of Expansion by Heat. By* Prof. W. E. AYRTON, *F.R.S., and* Prof. J. PERRY, *F.R.S.*[*]

THE apparatus consists of a metal tube within which the wire or rod, whose coefficient is to be determined, is placed. One end of the wire is rigidly attached to one end of the tube, and the other end connected to an Ayrton and Perry magnifying-spring, a pointer attached to which indicates the change of length due to alteration of temperature. Steam or water may be passed through the tube, the temperature of the wire being shown on a thermometer. The arrangement is very sensitive, and with a pointer about 20 centim. long the motion is magnified about 1000 times.

A magnifying-spring attached to an aneroid was also shown and its great sensibility demonstrated. A combination of a spring of large diameter and pitch with one of small diameter and pitch was exhibited. By such a combination small rotations can be immensely magnified. The great features of the patent spring as a magnifier are the entire absence of friction and back lash, and the large range of proportionality.

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[*] Read May 12, 1888.

III. *On a New Barometer, called "the Amphisbæna."*

By T. H. BLAKESLEY.*

THIS instrument consists of a straight glass tube of uniform internal cross section, closed at one end and open to the atmosphere at the other. A thread of quicksilver occupies a portion of the tube, the space between the quicksilver and the closed end being occupied by air. A uniformly graduated scale, of any convenient dimensions, *but whose zero point is coincident with the closed end*, is attached. Since there is no escape for the air contained between the quicksilver and the closed end, this scale serves to determine the volume of this air at any particular moment. The instrument can be suspended vertically with the closed end either upwards or downwards, and in these positions the volumes of the cushioned air, as indicated by the end of the quicksilver thread, can be ascertained. It is clear that when the closed end is upwards this air is subject to a pressure which is the difference between the atmospheric pressure and that due to a column of quicksilver equal in height to the length of the thread. But when the closed end points downwards, the pressure of the cushioned air is the sum of the atmospheric pressure and that due to the length of the thread.

These two observations, then, enable one to find the relation between the atmospheric pressure and that due to the thread of quicksilver.

Let H be the unknown height of the barometer;
l be the length of the thread of quicksilver;
A be the reading of the volume of air enclosed, when the closed end of the tube is upwards;
B be the reading of the volume, when the closed end is downwards.

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* Read June 23, 1888.

Then the product of the pressure and volume being the same in the two cases, assuming the temperature constant,

$$(H-l)A=(H+l)B,$$

or

$$H=\frac{A+B}{A-B}l.$$

Thus the following rule applies :—Divide the sum of the readings by their difference, and multiply by the length of the quicksilver thread.

Since the length of the quicksilver occurs as a factor, it is obviously advantageous that it should be a simple number, as 10 inches. Where centimetres are required, 25 centimetres or 50 centimetres would be convenient quantities.

Here it may be pointed out that the usual correction for temperature may be made once for all, by having the thread of quicksilver, not exactly 10 inches or 50 centimetres, but such a length as would be 10 inches or 50 centimetres at zero of temperature Centigrade. Then, by taking the simple number for use in the calculation, whatever the real length may, on account of temperature, be, the final result is the *reduced* barometer.

A convenient length for a portable instrument would be about 20 inches, with a 10-inch thread of quicksilver.

The diameter of the tube should be small enough to maintain continuity in the quicksilver. 1·2 millim. or one twentieth of an inch answers very well.

The value of an observation made with this instrument would theoretically not be injured should the quicksilver thread become, through mischance, discontinuous, so long as all the liquid remained in the tube, though it appears that practically the multiplication of ends by subdivision adds a certain amount of friction, and produces sluggishness in the movements of the quicksilver.

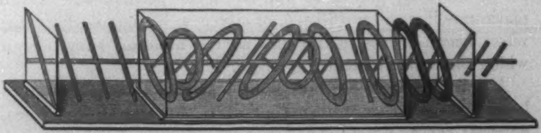
In using the instrument for determining mountain elevations, the length of the quicksilver may be left quite undetermined ; for the difference of level appears as a function of the *ratio* merely of the two pressures at the points of observation $\left(\mathfrak{H}\log\frac{p_1}{p_2}\right)$, where $\mathfrak{H}$ is the height of the homogeneous atmosphere. Therefore we can use the ratio of the functions $\left(\frac{\mathrm{A}+\mathrm{B}}{\mathrm{A}-\mathrm{B}}\right)$ of the *observations only* at the two stations.

In practice the glass tube is sunk into a groove in a light piece of wood provided with two eyes, which serve either for suspension from a nail in a wall, or for attaching a strap or cord for slinging the instrument round the shoulders in climbing. The eye at the open end of the tube will screw in far enough to stop the opening temporarily if that should be desirable; but if care is taken to climb with the closed end downwards, there is small fear of the quicksilver becoming either lost or discontinuous.

The instrument, as made by Messrs. Watson Brothers, of 4 Pall Mall, weighs between 6 and 7 oz.

IV. *On an Optical Model. By* Prof. A. W. RÜCKER, *F.R.S.*[*]

THE model exhibited and described is to illustrate the character of the vibrations in a crystal cut parallel to the axis, when plane-polarized light is incident upon it. A rectangular glass box represents the crystal, and glass plates placed at short distances from each end imitate crossed Nicols. A rod carrying coloured circular and elliptical rings and straight bars passes along the axis of the box. These rings are intended to indicate the character of the vibration at the different points at which they are placed. The length of the crystal is supposed to be such that plane-polarized red rays emerge plane-polarized in the initial plane after being successively plane, elliptical, circular, elliptical plane, elliptical circular, elliptical and plane-polarized within the crystal. All the light is quenched by the analyzing Nicol. Supposing another kind of light (say



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[*] Read June 23, 1888.

green) to be used, another rod with green ellipses &c. is placed in the box, and illustrates that such light may emerge elliptically polarized, one component only being stopped by the analyzer. This shows how plane-polarized white light, when passed through crystals placed between Nicols, may become coloured.

V. *A Design for a Standard of Electrical Resistance.* By J. A. Fleming, *M.A., D.Sc., Professor of Electrical Technology in University College, London*[*].

[Plate I.]

In designing a standard of electrical resistance the two points to which attention is directed are the choice of the material in which the standard is embodied, and the form or disposition of the instrument.

Experience is yet far from complete as to the entire permanence of wires of alloys over prolonged periods of time when employed as standards of electrical resistance; but having regard to the inconveniences which attend the use of mercury in standards intended to be conveyed about, evidence, as far as we have it, points to the tolerable permanence of the platinum-silver alloy (66 p.c. of silver + 32 p.c. of platinum) when drawn into wire, for use as the material substance of which the actual standard is made.

A definite length and gauge of standard wire has then to be so arranged that, whilst kept at a constant temperature, currents can be passed through it and the resistance between certain points ascertained.

The form which has hitherto been chiefly manufactured, and which is in most general use, is the form of standard which was designed by the Committee of the British Association on the original introduction of the B.A. unit, and shown in plate 4 of the 'Reprint of Reports on Electrical Standards,' by Prof. Fleeming Jenkin. In this form of standard the actual coil is wound on a bobbin consisting of a tube of thin brass having ebonite cheeks. Attached to these cheeks are the two long bent copper rods which serve as the electrodes,

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[*] Read November 10, 1888.

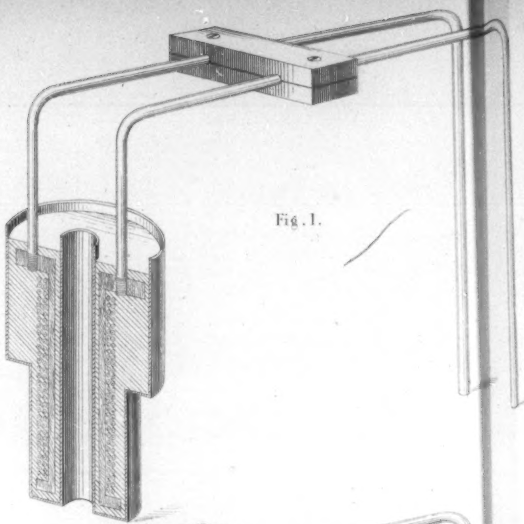

Fig. 1.

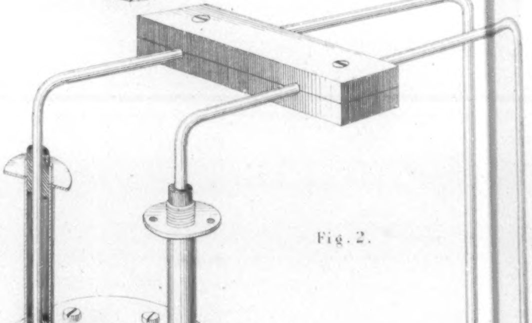

Fig. 2.

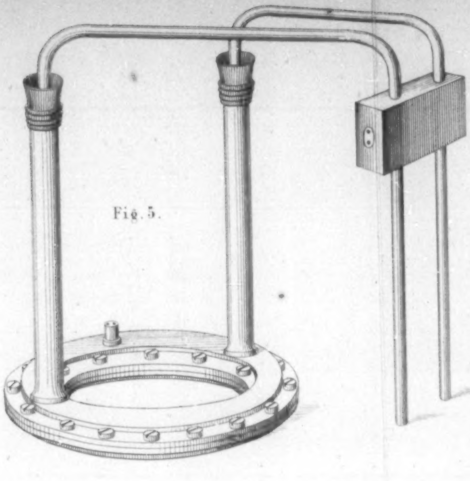

Fig. 5.

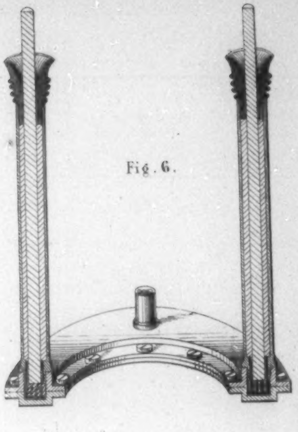

Fig. 6.

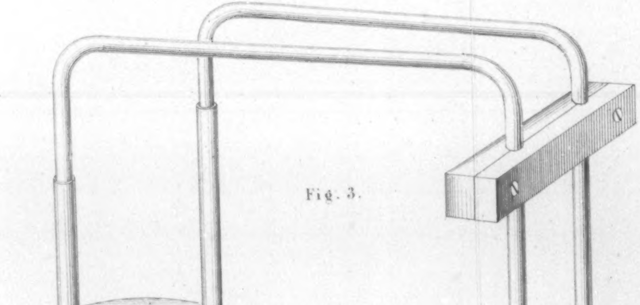

Fig. 3.

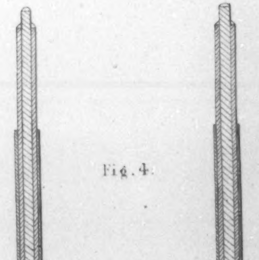

Fig. 4.

Mintern Bros lith.

held in position by a distance-piece of ebonite. In order that the coil may be immersed in a medium of known temperature it is further enclosed in a thin shell of brass consisting of a double tube (see fig. 1), and the whole shell filled up with paraffin wax or ozokerit. Some makers then place a thin lid of ebonite on the top of the shell.

Experience gained by a rather extensive use of standards of resistance of this form has indicated to the writer that this design can be, with some advantage, modified. The disadvantages of the present B.A. form of standard are as follows:—When in use the standards must be placed in water of a known temperature or in melting snow or ice. After a sufficiently prolonged time the temperature of this water can be taken, and the temperature of the water will be the temperature of the wire of the standard, assuming that equilibrium of temperature has been attained. If a current is now passed through the coil in order to take a measurement of its electrical resistance, the temperature of the wire is raised, and its resistance is altered.

Other things being equal, the best design of coil is that in which this electrically developed heat is got rid of by diffusion as quickly as possible. The embedding of a coil in a large mass of badly-conducting material like paraffin or ozokerit is, from this point of view, a great disadvantage.

Sufficient electrical insulation has to be provided ; but this should be achieved without the use of more enveloping insulation than necessary.

The two chief objections to the B.A. form of standard are, however, these :—

First, it cannot be placed in water with the shell wholly under water or under ice without short-circuiting the electrodes, and, when used as intended, whilst the narrow or bottom portion of the coil is in the water, the upper and more massive portion is in the air, and therefore may be at a different temperature to the bottom portion. Hence arises a doubt as to the actual temperature of the coil of wire. It has to be borne in mind that the limitation of accuracy in such comparisons of standards of resistance is determined by the difficulty of ascertaining temperature, and not in the mere measurement of resistance. Uncertainty as to the actual temperature of the wire to the extent of one or two tenths of a degree Centigrade renders nugatory elaborate arrangements for very accurate measurement of resistance.

Second. The standards, as at present constructed, are liable to another defect. If the standard is being used in melting ice or snow, and therefore cooled to 0° Cent., deposition of dew will take place upon the upper surface, whether the ebonite lid or paraffin-wax surface, through which the copper-rod electrodes protrude. The copper rods are originally lacquered or varnished, but when the lacquer wears off, any film of moisture so deposited will short-circuit the electrodes and reduce the observed resistance. In comparing standards in melting ice, either then the whole shell must be as far as possible placed under the melting ice, in which case stirring the liquid may splash water on to the surface of the paraffin, or else the shell has to be only partly immersed, in which case ambiguity exists as to the actual temperature of the coil of wire.

These and some other difficulties, such as that of keeping a rather deep vessel of melting ice at a constant temperature, have impressed on the writer the necessity for modifying the form of the standard, and one form which has proved itself to be very satisfactory in use is as follows :—The case or shell which contains the coil is in the form of a ring (see fig. 5). This ring consists of a pair of square-sectioned circular troughs provided with flanges which can be screwed together so as to form a square-sectioned, hollow, circular ring.

From this ring proceed upwards two brass tubes about five or six inches in length. Down these brass tubes pass the copper electrodes or rods, and these rods are insulated from the tubes at the top and bottom by ebonite insulators. The insulator at the bottom of the tube, where it enters the ring, is a simple collar, that at the top has the form of a funnel corrugated on its outer surface. The use of this funnel will be referred to presently. The actual resistance-coil is a length of platinum-silver wire three-fold silk-covered. The silk-covered wire is first baked above 100° C. to dry it completely, and then immersed in melted ozokerit or paraffin.

The so insulated wire is cut about the proper length and laid double or folded once upon itself and then rolled up on a wooden mandril so as to form a circular coil of diameter suitable to drop into the hollow of the brass ring. The wire being wound double, its coefficient of self-induction is rendered very small. This coil of wire is then wrapped over with white silk and again dipped in melted ozokerit. The ends of the wire are next soldered into nicks in the ends of the copper rods, they having been previously pushed a little way through the brass tubes for the purpose, and afterwards drawn back into proper positions. The coil is then packed into the circular groove, and, after adjusting the resistance to the proper value, the bottom half of the ring is placed over it. A thin washer of indiarubber is inserted between the flanges, and the whole screwed tightly together. The resistance-coil is thus enclosed in a thin ring of metal, and can be placed wholly below the surface of water or ice. In order to test the tightness of the joints, a little test-pipe is provided on the upper surface of the ring. By placing the ring coil below water and blowing into the test-pipe, the good fitting of the joints can be assured. The aperture of this test-pipe is afterwards closed by solder or a screw (see fig. 6).

Apart from the insulation of the coil itself it will be apparent that the insulation is limited by the amount of insulation resistance secured at the ebonite insulators at the top end of the brass tubes. Any leakage from the copper rod over these insulators to the brass tube destroys to that extent the insulation of the coil. The object of making these external insulators funnel-shaped is to prevent surface creeping, due to condensation of moisture on them, by placing paraffin oil or insulating liquid in the funnel-shaped cavity. When this is done, even if dew should collect on the outer surface of the funnels, the inner surface is kept dry by the paraffin oil placed in them, the action being the same as that in the well-known Johnson and Phillip's fluid insulator.

The ring-coils when in use are placed in rather shallow zinc troughs, which can be filled with water, and which are closed with a wooden lid. When so placed the whole of the actual coil or resistance part is down beneath the liquid at one level, where the temperature can be accurately ascertained. The insulators and point of emergence of the electrodes are away up above the level of the water, and well protected from any action which might permit of leakage over them.

The large metallic mass of the ring assists in bringing the resistance-coil quickly back to the temperature of the surrounding water, and the coil therefore "tests quickly." In all other respects these standards of resistance are as compact and portable, and not more expensive to construct than the old form of B. A. standard, whilst obviating the difficulties which present themselves in the use of the old form in very accurate comparisons of resistance.

Other forms of standard coil which have been tried are indicated in figs. 2, 3, and 4, but have not proved themselves to be as convenient as that above described and illustrated in figs. 5 and 6.

It is quite possible to have two or more coils of wire inside the same ring, each coil having its separate pair of electrodes. A useful coil of this form can be made up containing 1, 10, and 100 ohms, so that comparisons can be quickly made at the same temperature with these three multiples of the same unit of resistance.

The adjustment of the coils to a certain value presents no great difficulties. The wire is in the first instance cut a little longer than required, and its resistance nearly adjusted; when the two ends of the coil have been soldered to the lower ends of the copper rods, the resistance is again taken from the ends of the electrodes. This resistance should be a little greater than the final value required. The middle point of the wire or extreme loop is now stripped of its silk and the loop twisted up with the pliers, the resistance being carefully taken at intervals. When just a very little in excess of the value required the twisted coil is touched with solder, and having been bound over with insulating material the coil is completed. In the construction of standards it is obvious that it is not so important that the resistance should have an exact integer value at any temperature as that its value at some temperature and its coefficient of variation of temperature should be exactly known.

VI. *On the Upper Limit of Refraction in Selenium and Bromine.*

By Rev. T. PELHAM DALE, *M.A.*[*]

IN my former paper read before the Society[†] I showed that the value of the limit could be found by the solution of the equation

$$a \sin \theta = \sin m\theta\ ;$$

where a is the ratio of wave-lengths in free æther, and $\theta = \frac{\pi h}{l}$, h being what I there called the molecular distance, and l the corresponding wave-length within the medium. If $a \sin \theta$ be greater than unity, the solution is imaginary.

It was also shown that if ν be limit of refraction, μ the index corresponding to θ, that

$$\frac{\mu \sin \theta}{\theta} = \nu.$$

If $\theta = \frac{\pi}{2}$, we have

$$\mu = \frac{\pi}{2} \nu\ ;$$

μ being the index of the limit of refraction towards the violet end of the spectrum. Call this the upper limit, and denote it by μ_k.

$\therefore\ \mu_k - \nu$ is the total dispersion

$$= \left(\frac{\pi}{2} - 1\right) \nu\ ;$$

also

$$\frac{\mu_k - 1}{d} = \text{constant independent of temperature.}$$

Also by the relation

$$\frac{\pi h}{l} = \theta,$$

we have for l_k the upper limit

$$\frac{\pi h}{l_k} = \frac{\pi}{2},$$

or

$$2h = l_k.$$

Hence the quantity h is half the wave-length within the medium of the limiting transmissible ray.

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[*] Read November 10, 1888.

[†] Proc. Phys. Soc. vol. ix. p. 167.

As also

$$\sin\theta = \frac{\lambda}{\lambda_1}\sin\theta_1,$$

or

$$\frac{\lambda}{\lambda_1} = a,$$

we have at the limit

$$\frac{\lambda}{\lambda_k}\sin\theta = \sin m\theta,$$

$$= \sin\frac{\pi}{2} = 1\,;$$

$$\therefore\ \lambda\sin\theta = \lambda_k.$$

If on further examination this upper limit should be established as a physical entity it will modify our ideas of dispersion, which we must then regard as identical with refraction.

There is a relation between the Eulerian integral $\Gamma(n)$ and $\frac{\sin\theta}{\theta}$ which is worthy of notice,

$$n\pi = \theta.$$

By a well-known relation,

$$\Gamma n\,.\,\Gamma 1-n = \frac{\pi}{\sin n\pi}.$$

Hence

$$\Gamma(1+n)\Gamma(1-n) = \frac{n\pi}{\sin n\pi}.$$

If a table of $\Gamma(1+n)$ is at hand, it can be used as a practically expeditious method of finding the value denoted by θ*.

Also if for n we write $1-n$ in the above, we have

$$\Gamma(1+1-n)\big(\Gamma 1-(1-n)\big) = \frac{\pi - n\pi}{\sin(\pi - n\pi)},$$

or

$$\frac{1-n}{n}\Gamma(1+n)\ \Gamma(1-n) = \frac{\pi-\theta}{\sin(\pi-\theta)},$$

which gives us

$$\frac{\sin(\pi-\theta)}{\pi-\theta} = \frac{n}{1-n}\cdot\frac{\sin\theta}{\theta}.$$

Now since $\sin(\pi-\theta)$ is equally a solution of the equation, $a\sin\theta = \sin m\theta$, it is probable therefore that this solution may have a physical meaning.

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* The tables of logarithms by Vassal containing a column of circular measures of arc serve every purpose as far as calculation is concerned.

It was noticed in my former paper that, of the substances for which data were at hand, selenium alone had its upper limit within the visible spectrum. Accordingly a fresh calculation of the values of θ_k, ν, and λ_k were made and the results given below (Table I.).

It will be seen from the table that the limiting wave-length which is transmissible is $\lambda = 5295{\cdot}7$. This is a little below λ_K, which is 5269·13. It was of great interest to observe how far this purely theoretical result would be supported by observation. As neither temperature nor specific gravity of specimen were given, and the indices of refraction to the third place of decimals only, it was evident that, as a preliminary experiment, all that could be sought for was a general agreement.

With this in view I had a Browning amateur spectroscope-eyepiece fitted to my microscope, an old instrument constructed many years ago by Troughton and Sims. After a few trials I succeeded in obtaining a tolerably uniform film of selenium mounted as an ordinary microscope-slide. The film when cold was quite transparent, and transmitted a deep ruby tint. This film was found under the spectroscope to transmit rays nearly to the line D. Another and thinner slide was prepared. Under the microscope the thinner portions were found to be of a more orange tint, due to a spectrum reaching to the beginning of the green. Even in brilliant sunlight no sensible extension beyond this took place. In parts of the film were holes. If these were in the field they appeared as spectra extending to the violet end, but with sharp well-defined sides, showing apparently that a very thin film was effective to stop all the upper rays. Thus, then, the agreement of theory and observation seems as satisfactory as could be anticipated, considering both the data and instrumental means employed.

The examination of sulphur gave similar results. Here the limiting wave-length lies beyond the visible spectrum. A film of melted sulphur transmits yellow light. This film darkens rapidly as the temperature rises, and when the sulphur begins to boil, it, as is well known, assumes a brownish tint. Examined under the spectro-microscope, it is seen that the absorption is confined to the violet end. If the film be placed boiling under the spectro-microscope, the spectrum is seen to be generally darkened till near the red end, and the violet quite cut off. As the film cools, the resulting dark cloud seems to recede towards the violet. If the film be very thin it appears colourless to the eye, and under the spectroscope the violet is visible. It remains colourless after the sulphur has crystallized, appearing nearly white to the eye. Selenium also darkens rapidly by heat and then transmits a greyish light, which is probably due to its breaking up into crystals. It has, however, the property of becoming more transparent as it cools. Thus the optical properties of these two substances are as remarkably similar as are their chemical relations.

It will be observed that the quantity $\frac{\nu-1}{d}$, though by no means the same in both selenium and sulphur, are not far apart. There is, however, another relation which exists ; it is expressed by the formula, that in selenium,

$$\nu_{\mathrm{Se}}-1=(\nu_{\mathrm{S}}-1)\,\frac{\mathrm{E}}{d}\,;$$

where ν_{S} is limit of refraction of sulphur, E its equivalent, and d its density. That is, that the refraction-equivalent of selenium can be found by multiplying the refraction-equivalent of sulphur by the equivalent of selenium divided by the density of that element. This is worthy of remark, as it appears that the same relation within five or six units in the second place of decimals between isobutyl iodide as compared with isobutyl chloride, and orthobromotoluine compared with chlorotoluine and benzyl chloride. With regard to benzyl chloride, I may mention that calculation for the upper limit has revealed an error due to extracting the wrong logarithm of sin θ_{A}. The correct result given below agrees better with the rule that $\frac{\nu-1}{d}$ is constant in isomeric bodies. As in the case of two isomeric bodies, the equivalent is the same in both, it will be seen that this result is in agreement with the relation between selenium and sulphur stated above.

It is worthy of note that chlorine and bromine resemble sulphur and selenium in tint of transmitted light. The index of refraction for the line A in bromine is $\mu_A=1\cdot6260$. It occurred to me that it would be possible to obtain a probable idea of the spectrum of this element by assuming an upper limit of refraction. Taking this limit as D and F, to which I subsequently added G, I found that F gave $\theta_A=39° 44'$, $\nu=1\cdot4988$, and the refraction-equivalent 13·1. Dr. Gladstone, calculating from A, gives as a probable value 15·3, which would necessarily be greater than that obtained from the limit. A refraction-equivalent for ν obtained from orthobromotoluine showed that for this substance the refraction-equivalent for ν was between 14 and 15. If we assume G as the limit, then bromine comes out as 13·7. I subsequently procured a specimen of bromine. In as thin a film as I could produce, the spectra of bromine and selenium were almost indistinguishable; but the tint of bromine is decidedly more orange, and its vapour transmitted rays up to F certainly, and perhaps beyond; but in the brightest sunlight, and even when mixed with air, there was no indication of violet rays. So far as these very imperfect observations extend, they seem to support the conclusions previously arrived at by calculation. Without venturing, then, to speak at all decisively, the subject seems a promising one and likely to repay further investigation.

In the absence of data as to the spectrum of liquid chlorine, it is useless to make calculations as to any relations which might be found to subsist between it and bromine. It is, however, of importance to observe that its yellow tint is so similar to that of sulphur, that it is not improbable that the same portions of the spectrum would be affected in both. If this on further examination should turn out to be the fact, it would be one step more towards the attainment of the form of the function $\mu=\phi(\lambda)$, which would probably reveal relations of great interest toward determining the chemical constitution of bodies.

In the case of the critical angle the imaginary sine corresponds with the change from refraction to total reflexion. And this, again, to alteration of phase in the wave of the polarized ray. Is there anything similar in the limit of refraction? I think there is. If a mirror be constructed by melting selenium on a glass slide and then pressing on it a somewhat cooler glass, we shall obtain a reflecting surface on removing the glass, and also another on the corresponding surface attached to the glass. At angles near perpendicular incidence the reflected light has a certain greenish tint. This is more apparent on the side next the glass than on the free reflecting surface, but is very evident in both. It is no doubt to be expected that light not transmitted should in certain cases be reflected. If it be absorbed there must be an expenditure of energy within the medium, which we have every reason to believe acts on the wave-length within the medium and the velocity of its transmission. The importance of selenium is that it is a manageable substance, in which the critical point of transmission lies near the middle of the luminous spectrum. Bromine would no doubt prove equally interesting, but is by no means a pleasant substance to handle.

I should hardly have ventured to come before the Society with these very rough experiments, had I not hoped that some who possessed the instrumental means would be induced to make the observations. For my own part I am more than content with the humble though somewhat laborious office of computer between mathematical investigator and observer.

TABLE II.—Comparison of Sulphur and Selenium.

Selenium.						
Density. d.	Lower index. μ_A.	Upper index. μ_D.	Arc. θ_A.	Lower limit. ν.	$\frac{\nu-1}{d}$.	Shortest wave-length. λ_L.
4·3	2·653	2·980	44° 9′ 0″	2·3976	·32502	·53957
Sulphur.						
*d.	μ_A.	μ_E.	θ_A.	ν.	$\frac{\nu-1}{d}$	
d' 2·07 d'' 1·98	1·9024	1·9827	26° 32′ 0″	1·8610	·4164 ·4353	

* d' is the density of native sulphur.
d'' ,, ,, of that from fusion.

Thus we have the following empirical relation. If E be the equivalent of sulphur, and ν its limit of refraction,

$$\frac{\nu_r-1}{d'}E = 18\cdot36,$$

$$\frac{\nu_r-1}{d''} = 18\cdot32.$$

Again, we have

Orthobromotoluine, $\nu_r - 1 = \cdot529$,

Ref. equiv. Benzyl chloride $\frac{\nu_r-1}{d}E = 53\cdot47$,

do. do. Chlorotoluine, $\frac{\nu_r-1}{d}E = 59\cdot72$,

and

Isobutyl iodide, $\nu_r - 1 = \cdot479$,

Ref. equiv. Isobutyl chloride, $\frac{\nu_r-1}{d}E = 41\cdot69$.

TABLE III.—Bromine.

Spec. grav.	μ_A.	Assumed upper limit.		
		μ_D.	μ_F.	μ_G.
2·985	1·628	give 18·29 corresponding refraction equivalents.	18·31	18·37

Note on these Tables.—They can only be accepted as approximate, but the calculations have been carried quite as far as the uncertainty of the original data warrant. Under these circumstances the coincidences noticed between sulphur and selenium are of no great value, except perhaps as pointing out a likely direction in which the search for these empirical relations amongst the refractive indices should be pursued.

VII. *On the Calculation of the Coefficient of Mutual Induction of a Circle and a Coaxal Helix.* *By* Prof. J. V. JONES, *M.A., Principal of the University College, Cardiff**.

IF M is the coefficient of mutual induction of any two curves, we have

$$M = \iint \frac{\cos \epsilon}{r} \, ds \, ds' ;$$

where $r =$ the distance between two elements ds and ds', and $\epsilon =$ the angle between them.

Let us take for the equations of the circle and helix :—

$$\left.\begin{aligned} y &= a \cos \theta, \\ z &= a \sin \theta, \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(i.)}$$

$$\left.\begin{aligned} y' &= \mathrm{A} \cos \theta', \\ z' &= \mathrm{A} \sin \theta', \\ x' &= k\theta'. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(ii.)}$$

Then

$$M = \iint \frac{dx\,dx' + dy\,dy' + dz\,dz'}{\sqrt{(x-x')^2 + (y-y')^2 + (z-z')^2}},$$

$$= \int_0^{2\varpi} \int_0^{\Theta} \frac{\mathrm{A}a \cos(\theta - \theta') d\theta \, d\theta'}{\sqrt{\mathrm{A}^2 + a^2 - 2\mathrm{A}a \cos(\theta-\theta') + k^2\theta'^2}}.$$

If we change the variables in this integral, putting

$$\theta - \theta' = \phi,$$

$$\theta' = \phi',$$

we find

$$M = \int_{-\Theta}^{2\varpi-\Theta} \int_{-\phi}^{\Theta} \mathrm{V} \, d\phi \, d\phi' + \int_{2\varpi-\Theta}^{0} \int_{-\phi}^{2\varpi-\phi} \mathrm{V} d\phi \, d\phi'$$

$$+ \int_0^{2\varpi} \int_0^{2\varpi-\phi} \mathrm{V} d\phi \, d\phi', \quad . \quad \text{(I.)}$$

where

$$\mathrm{V} = \frac{\mathrm{A}a \cos \phi}{\sqrt{\mathrm{A}^2 + a^2 - 2\mathrm{A}a \cos \phi + k^2\phi'^2}}$$

$$= \frac{\mathrm{A}a \cos \phi}{\sqrt{\alpha^2 + k^2\phi'^2}},$$

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* Read November 10, 1888.

if
$$\alpha^2 = \mathrm{A}^2 + a^2 - 2\mathrm{A}a\cos\phi,$$

$$= \frac{\mathrm{A}a\cos\phi}{\alpha}\left\{1 - \frac{1}{2}\frac{k^2\phi'^2}{\alpha^2} + \frac{1\,.\,3}{2\,.\,4}\frac{k^4\phi'^4}{\alpha^4} - \&\text{c}.\right.$$

Now
$$\int \mathrm{V}\,d\phi' = \frac{\mathrm{A}a\cos\phi}{\alpha}\left[\phi' - \frac{1}{2}\cdot\frac{1}{3}\frac{k^2}{\alpha^2}\phi'^3 + \frac{1}{2}\cdot\frac{3}{4}\cdot\frac{1}{5}\frac{k^4}{\alpha^4}\phi'^5 - \&\text{c}.\right]$$

$$= \frac{\mathrm{A}a\cos\phi}{\alpha}[\mathrm{S}_{\phi'}],$$

where $\mathrm{S}_{\phi'}$ = the series in brackets.

[It may be noticed that

$$\mathrm{S}_{\phi'} = \frac{1}{k}\log\left(k\phi + \sqrt{\alpha^2 + k^2\phi^2}\right) - \frac{1}{k}\log\alpha.]$$

It may be shown that $\mathrm{S}_{\phi'}$ is convergent if $k\phi' < \alpha$. Now the maximum value of ϕ' is Θ, and the minimum value of α is $\mathrm{A} - a$. Hence the series is convergent for all values of ϕ' and α that occur in the second integration if $k\Theta < \mathrm{A} - a$. So long as this is the case we shall arrive, after performing the second integration, at a convergent series.

Substituting the above value of $\int \mathrm{V}\,d\phi'$ in equation (I.), we have

$$\mathrm{M} = \int_{-\Theta}^{2\varpi-\Theta} \frac{\mathrm{A}a\cos\phi}{\alpha}(\mathrm{S}_\Theta - \mathrm{S}_{-\phi})\,d\phi$$

$$+ \int_{2\varpi-\Theta}^{0} \frac{\mathrm{A}a\cos\phi}{\alpha}(\mathrm{S}_{2\varpi-\phi} - \mathrm{S}_{-\phi})\,d\phi$$

$$+ \int_{0}^{2\varpi} \frac{\mathrm{A}a\cos\phi}{\alpha}(2_{2\varpi-\phi} - \mathrm{S}_0)\,d\phi.$$

The sum of the last two of these integrals and the second half of the first is zero ; and hence

$$\mathrm{M} = \int_{-\Theta}^{2\varpi-\Theta} \frac{\mathrm{A}a\cos\phi}{\alpha}\mathrm{S}_\Theta\,d\phi$$

$$[\text{or } \mathrm{M} = \int_{-\Theta}^{2\varpi-\Theta} \frac{\mathrm{A}a\cos\phi}{\alpha}\log\frac{k\Theta + \sqrt{\alpha^2 + k^2\Theta^2}}{\alpha}d\phi].$$

If Θ is an integral multiple of ϖ (*i. e.* if $\Theta = n\varpi$), we may express M in a series of powers of $\left(\frac{kn\varpi}{\mathrm{A}+a}\right)^2$, the coefficients of which are functions of the quantity $\frac{2\sqrt{\mathrm{A}a}}{\mathrm{A}+a}$, and the complete elliptic integrals to that modulus.

For in this case

$$M=\int_{-n\varpi}^{2\varpi-n\varpi}\frac{Aa\cos\phi}{\alpha}\,S_\theta\,d\phi$$

$$=2\int_{0}^{\varpi}\frac{Aa\cos\phi}{\alpha}\,S_\theta\,d\phi.$$

The general term in this expression for M is

$$2(-1)^m\frac{1\,.\,3\,.\,5\,\ldots\,\overline{2m-1}}{2\,.\,4\,.\,6\,\ldots\,2m}\;\frac{k^{2m}(n\varpi)^{2m+1}}{2m+1}\,Aa\int_0^{\varpi}\frac{\cos\phi\,d\phi}{\alpha^{2m+1}}.$$

Now

$$\int_0^{\varpi}\frac{\cos\phi\,d\phi}{\alpha^{2m+1}}=\int_0^{\varpi}\frac{\cos\phi\,d\phi}{(A^2+a^2-2Aa\cos\phi)^{\frac{2m+1}{2}}}$$

$$=-\frac{2}{(A+a)^{2m+1}}\int_0^{\frac{\varpi}{2}}\frac{\cos 2\theta\,d\theta}{(1-c^2\sin^2\theta)^{\frac{2m+1}{2}}},$$

where $c=\dfrac{2\sqrt{Aa}}{A+a}$

$$=-\frac{2}{(A+a)^{2m+1}}P_m,$$

where $P_m=\displaystyle\int_0^{\frac{\varpi}{2}}\frac{\cos 2\theta\,d\theta}{(1-c^2\sin^2\theta)^{\frac{2m+1}{2}}}.$

Hence we may write

$$M=-\Sigma 4(-1)^m\frac{1\,.\,3\,.\,5\,\ldots\,\overline{2m-1}}{2\,.\,4\,.\,6\,\ldots\,2m}\;\frac{k^{2m}}{2m+1}\;\frac{(n\varpi)^{2m+1}}{(A+a)^{2m+1}}AaP_m$$

$$=-4n\varpi\frac{Aa}{A+a}\Sigma(-1)^m\frac{1\,.\,3\,.\,5\,\ldots\,\overline{2m-1}}{2\,.\,4\,.\,6\,\ldots\,2m}\;\frac{1}{2m+1}\left(\frac{x}{A+a}\right)^{2m}P_m,$$

where $x=kn\varpi$.

Or, writing down the first few terms of the series,

$$M=-4n\varpi\frac{Aa}{A+a}\left\{P_0-\frac{1}{2}\cdot\frac{1}{3}\left(\frac{x}{A+a}\right)^2P_1\right.$$
$$+\frac{1\,.\,3}{2\,.\,4}\cdot\frac{1}{5}\left(\frac{x}{A+a}\right)^4P_2$$
$$-\frac{1\,.\,3\,.\,5}{2\,.\,4\,.\,6}\cdot\frac{1}{7}\left(\frac{x}{A+a}\right)^6P_3$$
$$+\frac{1\,.\,3\,.\,5\,.\,7}{2\,.\,4\,.\,6\,.\,8}\cdot\frac{1}{9}\left(\frac{x}{A+a}\right)^8P_4$$
$$\left.-\ \&c.\right\}.$$

It now remains to indicate how P_0, P_1, P_2, &c. may be calculated.

Let

$$Q_m = \int_0^{\frac{\pi}{2}} \frac{d\theta}{(1-c^2\sin^2\theta)^{\frac{2m+1}{2}}}.$$

Then

$$P_m = \left(1-\frac{2}{c^2}\right)Q_m + \frac{2}{c^2}Q_{m-1}, \quad . \quad . \quad . \quad . \qquad \text{(II.)}$$

and

$$Q_m = Q_{m-1} + \frac{c}{2m-1}\frac{d}{dc}Q_{m-1}. \quad . \quad . \quad . \qquad \text{(III.)}$$

With the help of these two formulæ, observing that

$$Q_0 \ = F(c),$$

$$Q_{-1} = E(c),$$

and using the relations

$$\frac{dF(c)}{de} = \frac{E(c)}{c(1-c^2)} - \frac{F(c)}{c}$$

and

$$\frac{dE(c)}{de} = \frac{E(c)-F(c)}{c},$$

we may express P_0, P_1, P_2, &c. successively in terms of $F(c)$, $E(c)$, &c. The first three are :—

$$P_0 = \left(1-\frac{2}{c^2}\right)F + \frac{2}{c^2}E,$$

$$P_1 = \left(1-\frac{2}{c^2}\right)\frac{E}{1-c^2} + \frac{2}{c^2}F,$$

$$P_2 = \frac{2-c^2}{3c^2(1-c^2)}F - \frac{2(1-c^2+c^4)}{3c^2(1-c^2)^2}E.$$

I have not expressed P_3, P_4, &c. in terms of F, E, &c., because in practice it is easier to calculate them numerically in any special case as follows, rather than from their expressions in terms of F, E, &c.

(i.) Calculate

$$Q_1, \quad \frac{dQ_1}{dc}, \quad \frac{d^2Q_1}{dc^2}, \quad \frac{d^3Q_1}{dc^3}, \ \text{\&c.},$$

or

$$Q_1, \quad \dot{Q}_1, \quad \ddot{Q}_1, \quad \dddot{Q}_1, \ \text{\&c.}$$

These may be successively calculated from the equation

$$(1-c^2)Q_1 = E$$

and its derivatives :—

$$
\begin{aligned}
&c(1-c^2)\dot{Q}_1-2c^2Q_1=E-F,\\
&c(1-c^2)\ddot{Q}_1+\dot{Q}_1(1-5c^2)-3cQ_1=0,\\
&c(1-c^2)\dddot{Q}_1+4(1-2c^2)\ddot{Q}_1-13c\dot{Q}_1-3Q_1=0,\\
&\quad\&c.
\end{aligned}
$$

(ii.) Calculate Q_2, Q_3, Q_4, &c. from the equations :—

$$
\begin{aligned}
&Q_2=Q_1+\frac{c}{3}\dot{Q}_1,\\
&Q_3=Q_2+\frac{c}{5}\dot{Q}_2=Q_2+\frac{c}{5}\left\{\frac{4\dot{Q}_1}{3}+\frac{c}{3}\ddot{Q}_1\right\},\\
&Q_4=Q_3+\frac{c}{7}\dot{Q}_3=Q_3+\frac{c}{7}\left\{\frac{24}{15}\dot{Q}_1+\frac{11}{15}c\ddot{Q}_1+\frac{1}{15}c^2\dddot{Q}_1\right\},\\
&\&c.,
\end{aligned}
$$

which are obtained by successive applications of formula (III.).

(iii.) Calculate P_2, P_3, P_4, &c. from these values of Q_2, Q_3, Q_4, &c. by successive applications of formula (II.).

I have performed the calculations for a circle of 5 inches radius situated in the mean plane of a coaxal helix of 10 inches radius and axial length 4 inches.

In this case,

$$
\begin{aligned}
A&=10,\\
a&=5,\\
x&=2,\\
c&=\cdot942809\\
Q_{-1}=E&=1\cdot113741\\
Q_0=F&=2\cdot528625\\
Q_1&=10\cdot02366
\end{aligned}
$$

$$
\begin{aligned}
\dot{Q}_1&=156\cdot6023\\
\ddot{Q}_1&=5419\cdot795\\
\dddot{Q}_1&=156574\cdot9
\end{aligned}
$$

$$
\begin{aligned}
Q_2&=59\cdot23901\\
Q_3&=419\cdot7842\\
Q_4&=2207\cdot927
\end{aligned}
$$

$$P_0= \quad \cdot 54870$$
$$P_1= \quad 6\cdot 84018$$
$$P_2= \quad 51\cdot 4955$$
$$P_3= \quad 391\cdot 442$$
$$P_4=1613\cdot 68$$

Hence

$$\begin{aligned} M = n\{ & 54\cdot 86225 \\ & -1\cdot 69790 \\ & + \cdot 10226 \\ & - \cdot 00823 \\ & + \cdot 00041\} \\ = n \times & 53\cdot 25879 \end{aligned}$$

The above investigation was undertaken in connexion with a measurement of a resistance in absolute measure by the method of Lorenz, which I propose making in the Physical Laboratory at Cardiff. The standard coil which I have constructed for this purpose consists of a helix of copper wire, of diameter ·02 inch, wound in a screw-thread of pitch ·025 inch, cut in our Whitworth lathe on a brass cylinder of diameter 22 inches, the number of turns of wire being 185. For such a coil it seemed probable that Lord Rayleigh's formula, which is a first approximation, might not give a result of sufficient accuracy for the purpose in view; and when Lord Rayleigh, with whom I had the privilege of speaking on the matter last summer, expressed this opinion, it became clear that a formula embodying a closer approximation would have to be worked out. This might be done by considering the helix as a current-sheet, and proceeding to a second approximation on this hypothesis. But it seemed to me preferable to make a direct attempt at integration for the case of helix and circle, though I hardly anticipated that I should arrive in the result at a formula of such simplicity as that given above.

It is interesting to observe that, by Lord Rayleigh's formula, the coefficient of mutual induction of the circle and helix taken above for purposes of calculation comes out to be

$$n \times 53\cdot 317.$$

The difference between the two results is about one tenth per cent.

Prof. Perry asked if the thickness of the wire had been considered, and whether the author could give any information as to practical working formulæ for the mutual induction between two ordinary cylindrical coils in any position.

Dr. Fleming described a wooden anchor-ring wound like a gramme armature, and having a secondary coil added, which he had devised as a standard of mutual induction, and used for determining capacity absolutely.

In reply the Author said he had not considered the wire to have thickness, as he felt sure this would not affect the result for his coil by one part in 100,000. With respect to Dr. Fleming's anchor-ring, he considered the difficulty of winding it sufficiently uniformly to be a great drawback to its general adoption.

VIII. *On the Measurement of the Luminosity and Intensity of Light reflected from Coloured Surfaces. By* Capt. W. DE W. ABNEY, *C.B., R.E., F.R.S.*[*]

IN a communication[†] to the Royal Society General Festing and myself have shown how to compare the light of the different parts of the spectrum reflected from a white surface with that reflected from a coloured surface, and we gave the results of measurements of various colours, and from these constructed their luminosity curves by means of the luminosity curve of the spectrum of white light, which we had ascertained from our previous researches on Colour Photometry (Bakerian Lecture 1886). From the areas of these curves we deduced the total luminosity of these colours, compared with that of a white surface. Certain colours were combined by means of rotating sectors to form a grey, and this was matched with the grey formed by rotating sectors of black and white. By noting the angular value of each coloured sector, and of the black and white sectors, it was found that the luminosity curves of the colours were correct.

This is an indirect, though very accurate, method of ascertaining the luminosity of a coloured surface, and requires more time than can often be spared with that object in view. Professor O. Rood, in his "Modern Chromatics," indicates the manner in which he proceeds, which is as follows. He finds "that with the aid of rotating disks the second constant of colour (luminosity) can often be determined. Let us suppose we wish to determine the luminosity of paper painted with vermilion: a circular disk about six inches in diameter is cut from the paper and placed on a rotation apparatus . . On the same axis is fastened a double disk of black and of white paper, so arranged that the proportions of black and white can be varied at will. When the whole is set in rapid rotation the colour of the vermilion paper will of course not be altered, but the black and white will blend into a grey. This grey can be altered in its brightness till it seems about as luminous as the red." He then proceeds to give examples. This method must not only be difficult to manage, but also must be tedious before the match can be determined. A method based on the same plan is given later in this paper, which makes the matching of the luminosities more easy.

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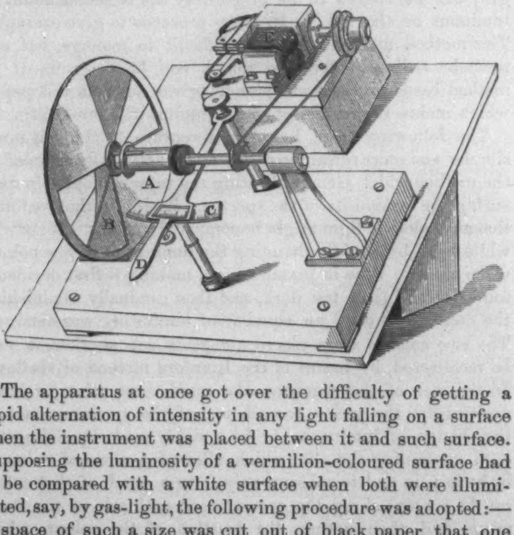
[*] Read November 24, 1888.

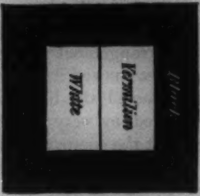
[†] "Colour Photometry," Part II., Phil. Trans. Part A, 1888.

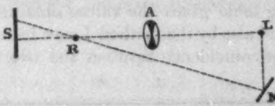
The following plan, however, I venture to think is much simpler and more certain in result, and is essentially founded on the method which General Festing and myself adopted in measuring the luminosity of the spectrum itself. In that we found that any coloured light might be compared with any other or with white light by *rapidly* changing the luminosity of one colour, when the two were in juxtaposition, making it first decidedly too light and then too dark, and then gradually diminishing the oscillations until an equality of luminosity was obtained. The two colours were placed alongside one another, it may be recollected, by means of the Rumford method of shadows. In the case of the spectrum the rapid diminution in luminosity from the yellow to each end of the spectrum enabled the change of luminosity to be quickly made by sliding the card containing the slit (which allowed a slice of coloured light to pass and to subsequently form a patch of that colour on the screen) along the spectrum on each side of the maximum, and then noting the position of the colour, which balanced a white light of known intensity. In the case of the measurement of the luminosity of coloured paper, this method evidently was inapplicable, and it remained to devise some other.

In our former experiments of the measurement of light reflected from coloured surfaces, General Festing and myself employed rotating sectors, movable when in motion, but the motion was slow, being effected by means of a screw. Mr. Hilger, in whose hands was placed a rough design for an improved set of rotating sectors, produced an instrument in which the sectors would be opened and closed rapidly by means of a simple lever arrangement without any screw-motion. The instrument is shown in the accompanying figure. A, B are the sectors which can be opened at pleasure by means of the lever, D, moving along an arc, C, on which is a scale of degrees. The sectors are rotated by a small electromotor, with sufficient speed to prevent scintillation.



The apparatus at once got over the difficulty of getting a rapid alternation of intensity in any light falling on a surface when the instrument was placed between it and such surface. Supposing the luminosity of a vermilion-coloured surface had to be compared with a white surface when both were illuminated, say, by gas-light, the following procedure was adopted:—A space of such a size was cut out of black paper that one side was rather less than twice the breadth of the rod used to cast a shadow. One half of the aperture was filled with a white surface and the other half with the vermilion-coloured surface. The light L was caused to illuminate the whole of this, and the rod R placed in such a position that it cast a shadow on the white surface, the edge of the shadow being placed accurately at the junction of the vermilion and white surfaces. A flat unsilvered mirror M was placed at such a distance and at such an angle that the light it reflected cast a second shadow on the vermilion surface. Between R and L were placed the rotating sectors A B. The white strip was then caused to be evidently too dark and then too light by altering the aperture of the sectors, and an oscillation of diminishing extent was rapidly made till the two shadows appeared equally luminous. A white screen was substituted for the vermilion, and again a comparison made. The mean of the two sets of readings of angular apertures gave the relative value of the two luminosities. It must be stated, however, that if the screen remained open, as represented, the values would not be correct, since any diffused light which might be in the room would relatively illuminate the white surface more than the coloured one. To obviate this the receiving screen was placed in a box in the front of which a narrow aperture was cut just wide enough to allow the two beams to reach the screen. An aperture was also cut at the front angle of the box through which the observer could see the screen. When this apparatus was adopted its efficiency was seen from the fact that when the apertures of the rotating sectors were





closed the shadow on the white surface appeared quite black, which it would not have done had there been any diffused light in any quantity present within the box. The box, it may be stated, was blackened inside and was used in a darkened chamber. The mirror arrangement was useful, as any variation in the direct light also showed itself in the reflected light. Instead of gas-light, reflected sky-light, the electric light, or sunlight can be employed by very obvious artifices; for it must be remembered that the comparison-light may be of any kind, and distinct, if necessary, from the light illuminating the coloured surface.

General Festing and myself had measured with great care the luminosities of emerald green, vermilion, and ultramarine, which we had combined in a rotation apparatus to produce a grey. The total luminosity of these three colours I measured by the apparatus described, using as the source of light to illuminate the colour, the patch of white light formed by a recombination of the spectrum, the comparison light being an ordinary gas-light.

The following table gives the values obtained by the spectrum method and also by the method I now introduce. It will be seen that the coincidence between the two values is very close:—

Colour.	Area of Luminosity Curves (from Paper in the Phil. Trans.).	Luminosity Readings by New Method.		Value from areas when white =100.	Value from New Plan when white =100.
		Readings.	Mean.		
White	534	73 77, 75, 76	75	100	100
Emerald Green .	221	32, 28, 29, 31, 31	30·2	41·1	40·3
Vermilion	216	33, 30, 29, 29	30·2	40·0	40·3
French Ultramarine........	49	6, 7, 7, 7·5	6·9	9·2	9·2

For another purpose I had measured the luminosity curves of six different colours by the spectrum method, and below is a table of the intensities of the light of different wave-lengths reflected from each and from a white surface. The areas indicate the total luminosities of the several colours.

Intensity Curves.

Scale No.	Yellow.	Orange.	Red.	Blue.	Green.	Neutral Tint.	White.
43	60	80	75	0	6	37	100
44	74	88	80	4	14	41	100
45	90	90	90	7	22	43	100
46	96	84	80	12	28	43	100
47	94	70	40	16	34	40	100
48	90	50	16	21	40	39	100
49	80	40	10	26	44	39	100
50	66	30	6	32	46	40	100
51	52	22	5	41	40	40	100
52	40	17	4	51	32	40	100
53	38	13	4	56	24	38	100
54	35	10	3	59	17	35	100
55	32	6	3	60	13	32	100
56	31	3	2	57	10	29	100
57	31	2	2	53	8·5	27	100
58	30	1	2	50	8	25	100

From this table are deduced the luminosity curves which are in the following table, the luminosity curve of the white being that given in the Bakerian lecture for 1886.

Luminosity Curves.

Scale No.	Yellow.	Orange.	Red.	Blue.	Green.	Neutral Tint.	White.
43	1·5	2·0	1·8	0	1·2	·9	2·5
44	12·2	16·5	13·2	·6	2·3	6·7	16·5
45	58·5	58·5	58·5	4·5	14·3	28·0	65·0
46	90·2	79·0	75·2	11·2	26·3	40·4	94·0
47	94·0	70·0	40·0	16·0	34·0	40·0	100·0
48	84·6	47·0	15·0	19·7	37·6	36·7	94·0
49	64	32·0	8·0	20·8	35·2	31·2	80·0
50	28·7	13·0	2·6	13·9	20·2	17·4	43·5
51	10·0	4·4	·9	8·0	7·7	7·8	19·4
52	3·8	1·8	·4	4·9	3·0	3·8	9·6
53	2·0	·7	·3	3·2	1·4	2·2	5·8
54	1·2	·4	·2	2·1	·3	1·3	3·6
55	·7	·1	·1	1·2	·2	·7	2·1
56	·4	...	...	·7	·1	·4	1·3
57	·3	...	...	·5	·1	·3	1·0
58	·2	...	...	·3	...	·2	·6
Areas...	453	323	217	107	183	218	534

[The scale-numbers which apply to the Fraunhofer-lines are as follows :—A = 42·56, B = 43·34, C = 44·26, D = 46·54, E = 49·62, F = 52·08, G = 57·72.]

The areas of the curves are those shown in the table. Taking the area of the luminosity curve of white as 100, the following are the luminosities of the colours derived from the above:—

White.	Yellow.	Orange.	Red.	Blue.	Green.	Neutral Tint.
100	84	60	41	20	34	41

The readings by the new method were:—

White.	Yellow.	Orange.	Red.	Blue.	Green.	Neutral Tint.
77	65	45	31	15·5	27	31

Taking white as 100, as before, the readings reduce to:—

White.	Yellow.	Orange.	Red.	Blue.	Green.	Neutral Tint.
100	84	58	40	20	35	40

The exactitude of the latest method is again exemplified, but it is not pretended that it is quite so accurate as the older method.

The method described is particularly adapted to a coloured surface of small dimensions. Where, however, a circle of some 3 or 4 inches in diameter can be utilized, the following modification of Rood's method may be adopted. Behind the sectors and on the spindle of the rotating disks, a disk of white card can be placed. The sectors being blackened on the other side, when revolved we can produce any shade of grey, from half black and half white to all black. On the other side of the sectors can be placed a smaller disk of coloured card, to be measured, and this will rotate with the sectors. Should the colour be a fairly dull one, the match of luminosity can be made by altering the shade of grey by rapid oscillation in aperture of the sectors, and the luminosity of the colour in terms of white determined when the amount of white light reflected from the black surface has been measured. Should the colour be more luminous than the grey given by half black and half white, the coloured disk can be toned down by means of a black semi-disk, adding a known quantity of the black surface by Maxwell's plan. The luminosities of the surfaces are then measured, and the true luminosity of the coloured surface in terms of the white surface, calculated after allowing for the added black.

This same plan may be adopted for measuring the *intensity* of the light of the various parts of the spectrum reflected from a coloured surface, in comparison with that reflected from a white surface. Colour patches of monochromatic light are formed by the method given in the Philosophical Magazine for 1885 (vol. xx.), which fall on the coloured disk and the rotating sectors, which form a grey in white light. There is less difficulty in this case than in ascertaining the luminosity, as the colour falling on the mixture of black and white is the same as that falling on the pigment. In this case, too, it may be necessary to add a fixed amount of black to the coloured disk in order to get a reading.

In reply to questions, Capt. Abney said the spectrum-method was the more accurate and could be relied on to 1 per cent. The new method gave results within 2 per cent., showing that the eye is very sensitive to small changes of luminosity when such changes take place in rapid succession.

IX. *On the Suppressed Dimensions of Physical Quantities.*

By A. W. Rücker, *M.A.*, *F.R.S.**

In the calculation of the dimensions of Physical quantities we not unfrequently arrive at indeterminate equations in which two or more unknowns are involved. In such cases an assumption has to be made, and in general that selected is that one of the quantities is an abstract number. In other words the dimensions of that quantity are *suppressed.*

The dimensions of dependent units which are afterwards deduced from this assumption are evidently artificial, in the sense that they do not necessarily indicate their true relations to length, mass, and time. They may serve to test whether the two sides of an equation are correct, but they do not indicate the mechanical nature of the derived units to which they are assigned. On this account they are often unintelligible.

Another difficulty is caused by the fact that the units which we find it convenient to use are sometimes themselves of an artificial character, and not such as would have been chosen had those who originally defined them possessed a clear conception of the nature of the phenomena with which they were dealing.

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* Read November 24, 1888.

The obstacles which the study of the subject presents to students would, I think, be greatly reduced if in developing it symbols were retained in the formulæ to represent the quantities the dimensions of which are suppressed, and if the artificial appearance of the dimensions of many quantities were more clearly traced to the artificial character either of the quantities themselves or of the assumptions on which these dimensions are calculated.

It is probable that, as the mechanical explanation of physical phenomena proceeds, the use of units based on simple mechanical conceptions will be extended, that some quantities now employed will be dispensed with or regarded from a different point of view.

Thus the satisfactory expressions of the dimensions of the various thermal units is not possible so long as there is any doubt as to the mechanical definition of temperature.

The equation

$$[\mathrm{M\,L^2\,T^{-2}}] = [\mathrm{J\,M}\,c\,\theta]$$

leads, if we regard c as a number, to the result

$$[\mathrm{J}] = [\mathrm{L^2\,T^{-2}}\,\theta^{-1}].$$

The quantity θ can hardly be taken as an abstract number, because the scale on which it is measured affects the numerical value of J, but, on the other hand, temperature as measured on the ordinary thermometer-scales has no relation to the units of length, mass, or time. No change in these units would affect the magnitude of the degree Centigrade or Fahrenheit. So long, therefore, as we adhere to the system of measuring heat in terms of calories instead of in ergs, temperature must be regarded as a fundamental quantity in the sense that the unit of temperature, though necessary for the expression of other thermal units, is itself independent of the units of length, mass, and time.

If it were sufficiently certain that the analogy offered by the theory of gases might be extended to all cases, it would follow that temperature depended only on mean energy of a particular kind (in gases the mean energy of translation) possessed by the molecules. The mean value of a quantity is of the same dimensions as the quantity itself, and thus temperature might be regarded not only as being measured by, but as being the mean energy of translation of a molecule, or of a standard number of molecules. The dimensions of temperature would thus be $[M L^2 T^{-2}]$. The number which expressed a given temperature would depend on the fundamental units, because they determine the magnitude of the unit of energy. It would be independent of the concrete mass or velocity of the molecules of any particular gas.

In the case of air at 0° C. and at atmospheric pressure $V = 48{,}500$ centim. per second. If then, *exempli gratiâ*, the unit of temperature were that necessary to produce an increase of 1 erg in the mean total energy of translation of the number of molecules contained in 1 cub. centimetre of gas at 0° C. and at atmospheric pressure, the absolute temperature of melting ice on this scale would be

$$\tfrac{1}{2} \times {\cdot}001293 \times 48500^2 = 1{\cdot}5207 \times 10^6.$$

Absolute temperatures Centigrade could be converted into this scale with considerable accuracy by the multiplier $10^5/18$; for, dividing $1{\cdot}5207 \times 10^6$ by this number, we get 273·7, which is the absolute temperature generally accepted as equivalent to 0° C. Heat absorbed by a gas at constant volume would be at once expressed in dynamical measure by the product of the change in temperature and of two abstract numbers, viz. the ratio of the number of molecules to the standard number, and the ratio of the increase of the total energy of a molecule to that of the energy of translation. Even if some such system should ultimately be adopted, our knowledge of the molecular construction and dynamics of solids and liquids is as yet too imperfect to warrant an attempt to place thermal units on a natural and mechanical basis. Until such a reform can be carried out, I think it would be best to place temperature in a class of *secondary fundamental units*, which, owing either to our ignorance or to our artificial methods of measurement, cannot be expressed in terms of length, mass, and time, and which must therefore be regarded as fundamental to other derived units which depend upon them.

The thermal units could then be tabulated in a more systematic manner than is usual, and the arbitrary character of the system would be emphasized by the symbol employed to represent the dimensions of temperature. If θ be the symbol chosen, if specific heat be taken as a number, and the unit of heat be defined with reference to the unit mass of water, the following results are obtained :—

Quantity of heat	$[M\theta]$
Mechanical equivalent of heat . . .	$[L^2 T^{-2} \theta^{-1}]$
Specific heat	1
Latent heat	$[\theta]$
Thermal capacity	$[M]$
Coefficients of expansion	$[\theta^{-1}]$
Coefficients of absorption and emission	$[ML^{-2} T^{-1}]$
Coefficient of conductivity	$[ML^{-1} T^{-1}]$
Entropy	$[M]$

Gravitation units hardly come within the scope of the present paper, but as they are important in their applications to thermal quantities I may indicate a method of dealing with them.

We must distinguish between the nominal unit of mass in such a system (M) and the real unit $(M\gamma)$, where γ is the abstract number which expresses the acceleration of gravity in terms of the units of length and time. The three fundamental units in such a system are length, time, and force. Mass is a derived unit subject to the condition that $[L T^{-2}]$ is a given concrete acceleration. The only practically important cases are those of force, work, power, and the mechanical equivalent of heat, and if we multiply the number expressing any one of these quantities in a gravitation system by (the abstract number) γ, and remember the condition that $\gamma\ [L T^{-2}]$ is constant, transformations can be effected as usual.

Thus, if J, J′, and J″ be the numerical values of the mechanical equivalent expressed in an absolute, and two gravitational systems, we have

$$J\ [L^2 T^{-2} \theta^{-1}] = J' \gamma'\ [L'^2 T'^{-2} \theta'^{-1}] = J'' \gamma''\ [L''^2 T''^{-2} \theta''^{-1}].$$

Thus to find the numerical value of J in the C.G.S. system when it is given in the English gravitational system,

$$J = 773{\cdot}2 \times 32{\cdot}2 \left(\frac{\text{foot}}{\text{centim.}}\right)^2 \left(\frac{\text{degree Cent.}}{\text{degree Fahr.}}\right) = 4{\cdot}16 \times 10^7.$$

To find J in the metre-kilogram-second gravitational system, when it is given in the British gravitational system, we reduce the last two of the above expressions by the relation $\gamma' [L' T'^{-2}] = \gamma'' [L'' T''^{-2}]$ to the form

$$J' [L' \theta'^{-1}] = J'' [L'' \theta''^{-1}];$$

so that

$$J = 773{\cdot}2 \left(\frac{\text{foot}}{\text{metre}}\right) \left(\frac{\text{degree Cent.}}{\text{degree Fahr.}}\right) = 424.$$

To reduce power from an absolute to a gravitational system, or *vice versâ*, we have

$$n_1 [M_1 L_1^2 T_1^{-3}] = n_2 [\gamma_2 M_2 L_2^2 T_2^{-3}].$$

Thus to find the number of ergs per second which correspond to one H.P.,

$$n = \frac{33000}{60} \times 32{\cdot}2 \left(\frac{\text{pound}}{\text{gram}}\right) \left(\frac{\text{foot}}{\text{cm.}}\right)^2 = 7{\cdot}46 \times 10^9.$$

It is noticeable that although the statement that J is independent of the unit of mass is in general true, it holds good only when the unit of heat and the unit of force are defined with respect to the same unit of mass. In a gravitational system this is not really the case, *e.g.* in the English system the unit of heat is defined with reference to the pound, while the employment of the gravitational measure of force involves a unit of mass=32·2 lbs. Hence, even when the nominal unit of mass has disappeared, the number which expresses the ratio between the real and nominal units on the gravitational system remains in the expression for a quantity such as J, which is based upon others which have themselves involved the introduction of mass.

The open admission in the symbols employed that the dimensions of some of the quantities are unknown is more important in the case of the electrical and magnetic than in that of the thermal units.

As Maxwell showed, the independent relations between these quantities are one less than the number required to determine them completely. The electrostatic and electromagnetic systems are therefore deduced from assumptions which are equivalent to suppressing the dimensions of inductive capacity and magnetic permeability respectively. It would, I think, be better to retain in the tables of dimensions symbols which represented these quantities as secondary fundamental units. The student would thus have brought more clearly before him the cause of the difference of the dimensions in the two systems, and the factors necessary to transform from the one system to the other would be indicated.

Thus if, instead of suppressing the dimensions of inductive capacity, we treat it as a secondary fundamental unit of unknown dimensions, we have the equations

$$[\mathfrak{D}\,\mathfrak{E}]=[\mathrm{M\,L^{-1}T^{-2}}],\quad [\mathfrak{D}]=[\mathrm{K}\,\mathfrak{E}];$$

whence

$$[\mathfrak{D}]=[\mathrm{M^{\frac{1}{2}}\,L^{-\frac{1}{2}}\,T^{-1}\,K^{\frac{1}{2}}}];$$

$$[\mathfrak{E}]=[\mathrm{M^{\frac{1}{2}}\,L^{-\frac{1}{2}}\,T^{-1}\,K^{-\frac{1}{2}}}].$$

In a precisely similar fashion we get

$$[\mathfrak{B}]=[\mathrm{M^{\frac{1}{2}}\,L^{-\frac{1}{2}}\,T^{-1}}\,\mu^{\frac{1}{2}}],$$

$$[\mathfrak{H}]=[\mathrm{M^{\frac{1}{2}}\,L^{-\frac{1}{2}}\,T^{-1}}\,\mu^{-\frac{1}{2}}].$$

By means of these relations all the units can be expressed in terms of M, L, T, K and M, L, T, μ, and the dimensions of M, L, T are those of the electrostatic and electromagnetic systems, according as the first or second mode of expression is adopted.

For since

$$[\mathfrak{D}]=[e\,\mathrm{L^{-2}}],\quad \therefore\quad [e]=[\mathrm{M^{\frac{1}{2}}\,L^{\frac{3}{2}}\,T^{-1}\,K^{\frac{1}{2}}}];$$

and since

$$[\mathfrak{B}]=[m\mathrm{L^{-2}}],\quad \therefore\quad [m]=[\mathrm{M^{\frac{1}{2}}\,L^{\frac{3}{2}}\,T^{-1}}\,\mu^{\frac{1}{2}}];$$

and from the dimensions of $[e]$ and $[m]$ those of all the other units can be found.

Maxwell has given a Table in which the dimensions of each of the electrical and magnetic units are expressed in terms of M, L, T, and of either electrical quantity or the strength of a magnetic pole. My suggestion amounts to nothing more than that we should use a similar list in which the unknown quantities are K and μ instead of e and m. The dimensions in terms of L, M, and T will then be those in the electrostatic and electromagnetic systems, and, instead of suppressing K and μ, we should retain them as representing dimensions which are unknown in terms of the fundamental units.

In cases where it is necessary to indicate whether the quantity represented by a symbol is understood to be expressed in terms of K or μ, we may use the letters *s* and *m* subscript to distinguish between them. Thus e_s and e_m are not quantities whose dimensions are fundamentally different, but quantities the dimensions of which are unknown and are expressed in different ways in terms of other unknowns. The letters *s* and *m* indicate which of the two methods which are in practical use is employed.

By equating two such expressions for the same quantity, or by using any general relation between any two of the derived quantities, we get a necessary relation between the dimensions of the secondary fundamental units in terms of the primary fundamental units of length, mass, and time.

Thus we have from any one of Maxwell's equations (1) to (12) (p. 241) the general relations

$$[e\,m] = [\mathrm{M\,L^2\,T^{-1}}].$$

If we choose to express *e* in terms of K and *m* in terms of μ, the relation still obtains, and thus

$$[e\,m] = [\mathrm{M^{\frac{1}{2}}\,L^{\frac{3}{2}}\,T^{-1}\,K^{\frac{1}{2}}}] \times [\mathrm{M^{\frac{1}{2}}\,L^{\frac{3}{2}}\,T^{-1}}\,\mu^{\frac{1}{2}}] = [\mathrm{M\,L^2\,T^{-1}}];$$

$$\therefore \quad [\mathrm{K^{-\frac{1}{2}}}\,\mu^{-\frac{1}{2}}] = [\mathrm{L\,T^{-1}}].$$

It is obvious that there must be such a relation, for since only one equation is wanting for the complete determination of the dimensions in terms of L, M, and T, we cannot have two independent methods of expressing them.

If, then, we use the letters *s* and *m* subscript in the sense above explained,

$$[e_s\,m_m] = [\mathrm{M^{\frac{1}{2}}\,L^{\frac{3}{2}}\,T^{-1}\,K^{\frac{1}{2}}}] \times [\mathrm{M^{\frac{1}{2}}\,L^{\frac{3}{2}}\,T^{-1}}\,\mu^{\frac{1}{2}}],$$

$$[e_s\,m_s] = [\mathrm{M^{\frac{1}{2}}\,L^{\frac{3}{2}}\,T^{-1}\,K^{\frac{1}{2}}}] \times [\mathrm{M^{\frac{1}{2}}\,L^{\frac{1}{2}}\,K^{-\frac{1}{2}}}],$$

$$[e_m\,m_s] = [\mathrm{M^{\frac{1}{2}}\,L^{\frac{1}{2}}}\,\mu^{-\frac{1}{2}}] \times [\mathrm{M^{\frac{1}{2}}\,L^{\frac{1}{2}}\,K^{-\frac{1}{2}}}],$$

$$[e_m\,m_m] = [\mathrm{M^{\frac{1}{2}}\,L^{\frac{1}{2}}}\,\mu^{-\frac{1}{2}}] \times [\mathrm{M^{\frac{1}{2}}\,L^{\frac{3}{2}}\,T^{-1}}\,\mu^{\frac{1}{2}}]:$$

and if we employ the relation

$$[\mathrm{K^{-\frac{1}{2}}}\,\mu^{-\frac{1}{2}}] = [\mathrm{L\,T^{-1}}],$$

we see that the right-hand sides of these expressions are all equal to each other and to $[\mathrm{M\,L^2\,T^{-1}}]$.

In this case the secondary fundamental units disappear from the final expression. This would not always be the case for the product of any two units chosen haphazard, but it would always be true that any product or quotient of units could, by the relation

$$[K^{-\frac{1}{2}} \mu^{-\frac{1}{2}}] = [LT^{-1}],$$

be reduced to the same dimensions in terms of M, L, T, and either K or μ, whether they were or were not originally expressed in terms of one of these two last quantities only.

The method can be applied, not only to express more clearly that the absolute dimensions of the electrostatic and electromagnetic systems are the same, but to transform the numerical expressions for given concrete electrical or magnetic quantities from the one system of units to the other.

In order to transform a length from one scale to another by the relation $n_1[L_1] = n_2[L_2]$ we must know not only n_1 or n_2 but also the numerical value of the ratio L_1/L_2. In like manner, to transform electrical quantities from a system expressed in terms of K to a system expressed in terms of μ, we must know the numerical value of the ratio

$$K^{-\frac{1}{2}} \mu^{-\frac{1}{2}}/LT^{-1}.$$

If, then, v be the numerical value of the velocity of light expressed in terms of the units L and T, we can transform both dimensionally and numerically by the relation

$$[K^{-\frac{1}{2}} \mu^{-\frac{1}{2}}] = v[L\,T^{-1}].$$

Thus, to find the number of C.G.S. electrostatic units in y C.G.S. electromagnetic units of quantity, we have

$$\begin{aligned} x[L^{\frac{3}{2}} M^{\frac{1}{2}} T^{-1} K^{\frac{1}{2}}] &= y \times [L^{\frac{1}{2}} M^{\frac{1}{2}} \mu^{-\frac{1}{2}}]; \\ \therefore \quad x &= y[L^{-1} T K^{-\frac{1}{2}} \mu^{-\frac{1}{2}}] \\ &= y[L^{-1} T] \times v[L\,T^{-1}] = vy. \end{aligned}$$

To find the number of C.G.S. Electromagnetic Units of Resistance in 20 *C.G.S. Electrostatic units.*

Working this out from first principles we have, by Maxwell's equations (*loc. cit.*) (2) and (4),

$$[R] = \frac{[E]}{[C]} = [M\,L^2\,T^{-1}\,e^{-2}];$$

whence, substituting for e in turn in terms of K and μ,

$$[R] = [L^{-1}\,T\,K^{-1}] = [L\,T^{-1}\mu].$$

Hence

$$x[\mathrm{L}\mathrm{T}^{-1}\mu]=20[\mathrm{L}^{-1}\mathrm{T}\mathrm{K}^{-1}].$$

$$\therefore \quad x=20[\mathrm{L}^{-2}\mathrm{T}^{2}\mathrm{K}^{-1}\mu^{-1}].$$

And since

$$[\mathrm{K}^{-1}\mu^{-1}]=v^2[\mathrm{L}^2\mathrm{T}^{-2}],$$

$$x=20v^2,$$

which is correct.

In these cases the fundamental units of length, mass, and time, and the medium with respect to which the unit charge and the unit pole are defined, are the same in both systems of units; but the method can be extended to cases in which these conditions do not hold, and which are far more complex than those which are likely to occur in practice.

If the unit charge were defined to be such that two unit charges at a distance of 1 metre in a medium of which the inductive capacity was twice that of air, repelled each other with a force which would, in one second, produce in a centigram a velocity of 1 metre per second, we may find the number of ordinary electrostatic units to which it is equivalent. We have the general relation

$$x[\mathrm{L}_1^{\frac{3}{2}}\,\mathrm{M}_1^{\frac{1}{2}}\,\mathrm{T}_1^{-1}\,\mathrm{K}_1^{\frac{1}{2}}]=1\times[\mathrm{L}_2^{\frac{3}{2}}\,\mathrm{M}_2^{\frac{1}{2}}\,\mathrm{T}_2^{-1}\,\mathrm{K}_2^{\frac{1}{2}}];$$

$$\therefore \quad x=\left(\frac{\text{metre}}{\text{cm.}}\right)^{\frac{3}{2}}\left(\frac{\text{centigram}}{\text{gram}}\right)^{\frac{1}{2}}\left(\frac{\mathrm{K}_2}{\mathrm{K}_1}\right)^{\frac{1}{2}}$$

$$=10^3\times10^{-1}\times\sqrt{2}=100\sqrt{2}.$$

Thus if it were desired to find the number of C.G.S. electromagnetic units of charge in the unit electrostatic charge above defined, we should proceed as follows:—

$$x[\mathrm{L}_1^{\frac{1}{2}}\,\mathrm{M}_1^{\frac{1}{2}}\,\mu_1^{-\frac{1}{2}}]=1\times[\mathrm{L}_2^{\frac{3}{2}}\,\mathrm{M}_2^{\frac{1}{2}}\,\mathrm{T}_2^{-1}\,\mathrm{K}_2^{\frac{1}{2}}].$$

But

$$\mu_1^{-\frac{1}{2}}=v_1\,\mathrm{K}_1^{\frac{1}{2}}\,\mathrm{L}_1\,\mathrm{T}_1^{-1};$$

$$\therefore \quad xv_1[\mathrm{L}_1^{\frac{3}{2}}\,\mathrm{M}_1^{\frac{1}{2}}\,\mathrm{T}_1^{-1}\,\mathrm{K}_1^{\frac{1}{2}}]=1\times[\mathrm{L}_2^{\frac{3}{2}}\,\mathrm{M}_2^{\frac{1}{2}}\,\mathrm{T}_2^{-1}\,\mathrm{K}_2^{\frac{1}{2}}];$$

where, as before, quantities with 2 subscript refer to the new unit. Of course v_1 must be expressed in terms of centimetres and seconds, as these are the magnitudes of L_1 and T_1 respectively.

$$\therefore \quad x\times3\times10^{10}=100\sqrt{2}, \qquad \therefore \quad x=\frac{\sqrt{2}}{3}\times10^{-8}.$$

As a final example we may take the following problem, in which the data are chosen merely for the purpose of illustration:—Find the number of C.G.S. electrostatic units of magnetic induction in 10 electromagnetic units of a system in which the units of length, mass, and time are the metre, centigram, and second respectively, and in which the specific inductive capacity and the refractive index of the standard medium with reference to air are 2 and 1·5 respectively. Assume that $K^{-\frac{1}{2}}\mu^{-\frac{1}{2}}$ is proportional to the velocity of light in the medium.

We have already seen that

$$[\mathfrak{B}]=[M^{\frac{1}{2}}L^{-\frac{1}{2}}T^{-1}\mu^{\frac{1}{2}}]=[M^{\frac{1}{2}}L^{-\frac{3}{2}}K^{-\frac{1}{2}}].$$

Hence if a subscript refers to air,

$$x\,[(\text{gram})^{\frac{1}{2}}(\text{cm.})^{-\frac{3}{2}}K_a{}^{-\frac{1}{2}}]$$
$$=10\,[(\text{centig.})^{\frac{1}{2}}\,(\text{metre})^{-\frac{1}{2}}\,(\text{sec.})^{-1}\,\mu^{\frac{1}{2}}].$$

We may now make the substitution

$$K_a{}^{-\frac{1}{2}}=3\times10^{10}\mu_a^{\frac{1}{2}}\,(\text{cm.})\,(\text{sec.})^{-1}$$

or else calculating the velocity of light in the medium from the refractive index, we may write

$$\mu^{\frac{1}{2}}=\frac{1{\cdot}5}{3\times10^{8}}K^{-\frac{1}{2}}\,(\text{metre})^{-1}\,(\text{sec.}).$$

By the first substitution we get

$$x\times3\times10^{10}\,[(\text{gram})^{\frac{1}{2}}\,(\text{cm.})^{-\frac{1}{2}}\,(\text{sec.})^{-1}\,\mu_a{}^{\frac{1}{2}}]$$
$$=10\,[(\text{centig.})^{\frac{1}{2}}\,(\text{metre})^{-\frac{1}{2}}\,(\text{sec.})^{-1}\,\mu^{\frac{1}{2}}];$$

$$\therefore\ x=\frac{10^{-9}}{3}\left(\frac{1}{10}\right)\times\left(\frac{1}{10}\right)\times\left(\frac{\mu}{\mu_a}\right)^{\frac{1}{2}}$$
$$=\tfrac{1}{3}\times10^{-11}\times1{\cdot}5\left(\frac{K_a}{K}\right)^{\frac{1}{2}}=\frac{1}{2\sqrt{2}}\times10^{-11}.$$

By the second substitution,

$$x\,[(\text{gram})^{\frac{1}{2}}\,(\text{cm.})^{-\frac{3}{2}}\,K_a{}^{-\frac{1}{2}}]$$
$$=10\times\frac{1{\cdot}5}{3\times10^{8}}[(\text{centig.})^{\frac{1}{2}}\,(\text{metre})^{-\frac{3}{2}}K^{-\frac{1}{2}}]\ ;$$

$$\therefore\ x=\frac{1}{2\times10^{7}}\left(\frac{1}{10}\right)\times\left(\frac{1}{100}\right)^{\frac{3}{2}}\left(\frac{K_a}{K}\right)^{\frac{1}{2}}=\frac{1}{2\sqrt{2}}\times10^{-11}\ ;$$

which is the same result as before.

In such a problem the amount of time and thought saved by the method of dimensions extended to secondary fundamental units is considerable.

I think that these examples are sufficient to justify the change in the method of exhibiting the dimensions of electrical quantities which I advocate, apart from the great theoretical advantage of being able to write electrostatic and electromagnetic quantities as being of the same absolute dimensions.

I therefore give a Table in which the powers in which K and μ enter into the various electrical and magnetic quantities are shown.

If we put $K=1$ we get the ordinary electrostatic, if $\mu=1$ the ordinary electromagnetic dimensions.

Table of Dimensions.

	Symbol.	Dimensions in terms of	
		L, M, T, and K.	L, M, T, and μ.
Quantity of electricity . . .	e	$[L^{\frac{3}{2}} M^{\frac{1}{2}} T^{-1} K^{\frac{1}{2}}]$	$[L^{\frac{1}{2}} M^{\frac{1}{2}} \mu^{-\frac{1}{2}}]$
Electric potential	E	$[L^{\frac{1}{2}} M^{\frac{1}{2}} T^{-1} K^{-\frac{1}{2}}]$	$[L^{\frac{3}{2}} M^{\frac{1}{2}} T^{-1} \mu^{\frac{1}{2}}]$
Quantity of magnetism . .	m	$[L^{\frac{1}{2}} M^{\frac{1}{2}} K^{-\frac{1}{2}}]$	$[L^{\frac{3}{2}} M^{\frac{1}{2}} T^{-1} \mu^{\frac{1}{2}}]$
Electrokinetic momentum of a circuit	p		
Electric current	C	$[L^{\frac{3}{2}} M^{\frac{1}{2}} T^{-2} K^{\frac{1}{2}}]$	$[L^{\frac{1}{2}} M^{\frac{1}{2}} T^{-1} \mu^{-\frac{1}{2}}]$
Magnetic potential . . .	Ω		
Electric displacement . .	$\mathfrak{D}$	$[L^{-\frac{1}{2}} M^{\frac{1}{2}} T^{-1} K^{\frac{1}{2}}]$	$[L^{-\frac{3}{2}} M^{\frac{1}{2}} \mu^{-\frac{1}{2}}]$
Surface density			
Electromotive force at a point .	$\mathfrak{E}$	$[L^{-\frac{1}{2}} M^{\frac{1}{2}} T^{-1} K^{-\frac{1}{2}}]$	$[L^{\frac{1}{2}} M^{\frac{1}{2}} T^{-2} \mu^{\frac{1}{2}}]$
Magnetic induction	$\mathfrak{B}$	$[L^{-\frac{3}{2}} M^{\frac{1}{2}} K^{-\frac{1}{2}}]$	$[L^{-\frac{1}{2}} M^{\frac{1}{2}} T^{-1} \mu^{\frac{1}{2}}]$
Magnetic force	$\mathfrak{H}$	$[L^{\frac{1}{2}} M^{\frac{1}{2}} T^{-2} K^{\frac{1}{2}}]$	$[L^{-\frac{1}{2}} M^{\frac{1}{2}} T^{-1} \mu^{-\frac{1}{2}}]$
Strength of current at a point .	$\mathfrak{C}$	$[L^{-\frac{1}{2}} M^{\frac{1}{2}} T^{-2} K^{\frac{1}{2}}]$	$[L^{-\frac{3}{2}} M^{\frac{1}{2}} T^{-1} \mu^{-\frac{1}{2}}]$
Vector potential	$\mathfrak{A}$	$[L^{-\frac{1}{2}} M^{\frac{1}{2}} K^{-\frac{1}{2}}]$	$[L^{\frac{1}{2}} M^{\frac{1}{2}} T^{-1} \mu^{\frac{1}{2}}]$
Capacity	$\left[\frac{e}{E}\right]=[q]$	$[LK]$	$[T^2 L^{-1} \mu^{-1}]$
Coefficient of self-induction .	$\left[\frac{p}{C}\right]=[L]$	$[L^{-1} T^2 K^{-1}]$	$[L\mu]$
Specific inductive capacity . .	$\left[\frac{\mathfrak{D}}{\mathfrak{E}}\right]=[K]$	$[K]$	$[L^{-2} T^2 \mu^{-1}]$
Magnetic inductive capacity .	$\left[\frac{\mathfrak{B}}{\mathfrak{H}}\right]=[\mu]$	$[L^{-2} T^2 K^{-1}]$	$[\mu]$
Resistance	$\left[\frac{E}{C}\right]=[R]$	$[L^{-1} T K^{-1}]$	$[L T^{-1} \mu]$
Specific resistance	$\left[\frac{\mathfrak{E}}{\mathfrak{C}}\right]=[r]$	$[T K^{-1}]$	$[L^2 T^{-1} \mu]$

To these we may add certain units in which both electrical and thermal quantities occur.

Thus, using the relations :—

Thermoelectric height × temperature = E.M.F.,
or Specific heat of electricity × temperature = E.M.F.,
Π × current × time = heat,
or $J\Pi$ = E.M.F.,
we get the following results :—

	Symbol.		
Thermoelectric height . . Specific heat of electricity	$\left.\right\} kt$	$[L^{\frac{1}{2}} M^{\frac{1}{2}} T^{-1} K^{-\frac{1}{2}} \theta^{-1}]$	$[L^{\frac{3}{2}} M^{\frac{1}{2}} T^{-2} \mu^{\frac{1}{2}} \theta^{-1}]$
Coefficient of Peltier effect	Π	$[L^{-\frac{3}{2}} M^{\frac{1}{2}} T K^{-\frac{1}{2}} \theta]$	$[L^{-\frac{1}{2}} M^{\frac{1}{2}} \mu^{\frac{1}{2}} \theta]$
Geometrical capacity . .	$q' = \frac{q}{K}$	$[L]$	$[L]$

In conclusion, then, I think it would be well to introduce symbols for what I have called secondary fundamental units instead of suppressing their dimensions, for the following reasons :—

(1) We thereby generalize the method of determining by dimensional equations the relation between a change in the magnitude of the units and in the number of which a given quantity is expressed.

(2) In the case of the electrical and magnetic units the method indicates clearly the cause of the difference between the dimensions in terms of M, L, and T of the same quantity in the two systems.

(3) It indicates that the dimensions of different quantities in the same system, which are apparently the same with respect to M, L, T, really are or may be different, as their dimensions with respect to K or μ are different ;

$$e.g.\ [\mathfrak{D}] = [L^{-\frac{1}{2}} M^{\frac{1}{2}} T^{-1} K^{\frac{1}{2}}]$$
$$[\mathfrak{E}] = [L^{-\frac{1}{2}} M^{\frac{1}{2}} T^{-1} K^{-\frac{1}{2}}].$$

(4) It suggests an explanation of the artificial and unintelligible character of the electric and magnetic units. If the dimensions of K and μ were known, they would probably be simplified.

(5) Lastly, I think the symbols are thus made to express the limits of our knowledge and ignorance on the subject more exactly than if we arbitrarily assume that some one of the quantities involved is an abstract number.

Mr. Blakesley, in commenting on thermal units, strongly protested against the use of the word "*Therm*" as a name for the unit of heat. If used at all, it should be reserved for the unit of temperature. Referring to the choice of fundamental units, he reminded the Society that the dimensions of quantity expressed in the electrostatic or electromagnetic systems become identical, if the unit velocity be the velocity of light; and by choosing the unit of time as a suitable decimal part of a day, the relations between electrical and practical mechanical units could be simplified.

Prof. Carey Foster, after discussing the effects of defining specific heat as a ratio, or as a quantity of heat on the dimensions of temperature, pointed out that as Quantity of heat = Temperature × Entropy, the dimensions of temperature would be determinate if those of Entropy were found.

Prof. S. P. Thompson considered part of the difficulties of dimensional equations arose from the fact that no distinction was made between *scalar* and *vector* quantities. Thus the dimensions of work and moment of a force are given as $M L^2 T^{-2}$, whereas the true representation for the latter would be $M L^2 T^{-2}\sqrt{-1}$, because the line by which the force is multiplied is at right angles to the force. By similar considerations, Ampère's rule for the force between two current elements can be derived from the magnetic equation $\frac{m m'}{r^2} = f$; for, replacing m and m' by equivalent current elements $i\,ds$ and $i\,ds'$, the equation becomes

$$f = \frac{\sqrt{-1}\, i\, ds \sqrt{-1}\, i\, ds'}{r^2}$$

$$= \frac{-i^2\, ds\, ds'}{r^2}.$$

Referring to the use of *Therm*, Dr. Thompson concurred with the remarks of Mr. Blakesley, and thought the word "*calorie*" answered all requirements. He also considered that thermal equations were greatly simplified by always expressing specific heat in ergs.

Prof. Ayrton was of opinion that students experienced much greater difficulty in dealing with electrical units than with thermal ones, and thought part of this was due to the vague way in which some of the standard text-books treated the subject. With reference to the force exerted by quantities of electricity, Prof. Perry and himself had pointed out that specific inductive capacity must be taken into account, for, contrary to Faraday, they had found it to be different in different gases.

In reply, Prof. Rücker said he often explained the identity of the dimensions of work and moment of a force by considering moment as measured by the work done in rotating through unit angle, the dimensions of angle being zero as regards L, M, and T. He also pointed out that in Bayne's 'Thermodynamics' specific heats are always expressed in ergs.

In thanking Prof. Rücker for his interesting paper, the President expressed his conviction that, by paying attention to the points considered, the difficulties arising from the two systems of units would be considerably diminished.

X. *Note on a Modification of the Ordinary Method of Determining Electromagnetic Capacity. By* J. W. W. Waghorn, *D.Sc.*[*]

The object of the proposed modification of the ordinary well-known method is to enable the capacity of a condenser or other conductor to be determined with sufficient accuracy without the costly apparatus generally employed.

The usual process consists, as is well known, in reading the "throw," d_1, of a galvanometer-needle under the impulse due to the flow of a quantity of electricity which charges the condenser to the potential of a certain battery, and in afterwards reading the steady deflexion, d_2 (not very different from d_1), when the galvanometer is connected in series through a total resistance R.

[*] Read December 8, 1888.

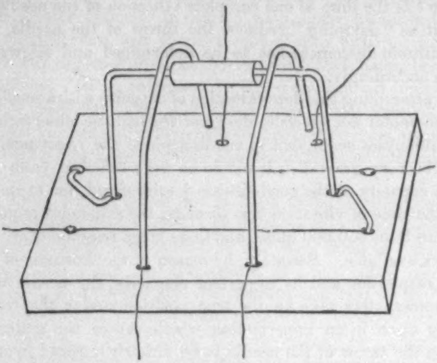
Then the required capacity, F (in farads) $= \frac{t \times d_1}{2\pi R d_2}$;
where t is the time of one complete vibration of the needle.

But as "damping" reduces the throw of the needle, the logarithmic decrement has to be determined and allowance made accordingly.

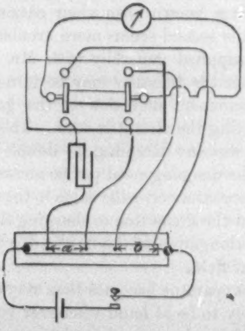
In attempting this determination of capacity with a sensitive galvanometer not specially designed for ballistic observations, the difficulties occur that in the first place the resistance, R, must be very great if d_1 is not to be very different from d_2; if the capacity of the condenser is ½ microfarad, for example, and the time of vibration two seconds, the resistance required is more than 600,000 ohms, and these large resistances are not always available. Secondly, by reason of the shortness of the fibre suspension and its imperfect elasticity, the needle does not immediately take up its true position under the forces acting upon it, an imperfection which causes the deflexion due to the throw of the needle to be unfairly lessened in comparison with the steady deflexion against which it is compared; the final result being that in ordinary cases the capacity of the condenser may work out one, two, or even four per cent. less than its true value.

The first difficulty, the use of very high resistances, can be avoided, as suggested by Prof. Ayrton, by reducing the E.M.F. of the battery in a known ratio when obtaining the steady deflexion; but the second seems more troublesome to avoid, unless we are supplied generally with Mr. Vernon Boys's quartz fibres; but the difficulty may be turned if, instead of reading the permanent deflexion of the galvanometer, the throw on completing the circuit is read. This throw would, of course, if there were no damping, be double the permanent deflexion, and the damping need not be allowed for, since it is (at least very approximately) the same in the two observations made; that is, in the throw due to charging the condenser and also in the first elongation of the needle due to the alteration in the permanent field.

The requisite apparatus becomes then very simple, and such as is always likely to be at hand whenever such observations are required, and involves merely resistance-coils of, say, from 1 to 10,000 ohms with wandering leads, a sensitive galvanometer, and battery. A Pohl's commutator or rocking key is very convenient, but of course not indispensable, for comparing in rapid succession the two throws which form the actual experiment.



The ordinary form of this commutator slightly modified, as shown, is very convenient in all the numerous electrical observations which require a comparison between any two deflexions, either steady or momentary, of an instrument such as a galvanometer or electrometer.



Let a be the resistance in that part of the resistance-coils between which the condenser circuit is applied, b the resistance in that part of the coils between which the permanent current is applied. The galvanometer itself forms a sufficient resistance to insert in the permanent-circuit branch, but its resistance must be known at the temperature at which it is used.

$$\text{F now} = \frac{t \times d_1 \times b}{\pi \times G \times d_2 \times a}.$$

The above expression is not strictly accurate, because damping occurs on an open circuit when the condenser is being charged, and on a closed circuit when the permanent current is flowing; but in a galvanometer with any approach to ballistic properties the error may be considered as negligible for ordinary purposes; for example, the logarithmic decrement of a galvanometer of 2000 ohms used in this operation on closed circuit was ·0128, on open circuit ·0415, involving an error of only one part in 2000, if the damping is entirely neglected in the two cases. With a Thomson galvanometer, in which the decrement reached the value of ·56 on open circuit, the error involved would, however, amount to one per cent.

The imperfect elasticity of the fibre may still exert a small influence, because the needle is swinging for a quarter period in the condenser charge and for a half period in the permanent-current observation; but if the error is not entirely obviated, it is brought down to very small limits.

Any sensitive galvanometer whose time of vibration exceeded two seconds, and in which there was not excessive damping, would probably be suitable for this determination; if the period is too small, among other difficulties, that of self-induction, delaying the passage of the permanent current, would occur.

With the apparatus connected as shown to a galvanometer whose resistance is known, the actual determination need not take more than two or three minutes, and involves but very little more trouble than is necessary to determine the capacity in terms of a standard condenser, without reposing the confidence which is required in that case. With a galvanometer not specially chosen for the purpose, the capacity of a half microfarad condenser, tested on several different occasions by different observers, gave ·497 for the lowest value and ·501 for the highest; but tested by the ordinary steady-deflexion method, the value determined was only ·485, the error in this case being due entirely to the imperfect elasticity of the short-fibre suspension.

The plan is not, however, suggested as a means of standardizing condensers where more refined means are available, but in the numerous cases where long-fibre ballistic galvanometers are not at hand and where an accuracy of, say, one half or one quarter per cent. is deemed sufficient.

Dr. Thompson pointed out that error frequently arises from the capacity of the key being appreciable where small condensers are being used, but the Author stated that the modification was not intended for such cases.

Prof. Ayrton remembered having used, in conjunction with Prof. Perry, the throw due to the permanent current instead of the steady deflexion, when experimenting with condensers containing E.M.F.s, and believed they abandoned it on account of the difference in decrement on closed and open circuits.

Mr. Sumpner regarded galvanometers with small differences in damping, on closed and open circuits, as unsuitable for ballistic purposes, and mentioned a case in which the latter was only half formed.

Mr. Boys considered the damping would be modified by having the condenser joined to the galvanometer-terminals, and thought the decrement should be decidedly different from that on pure open circuits.

In thanking the Author for his paper, the President said that any improved arrangements of known experiments or of lecture-apparatus would always be gladly received; and reminded members that in bringing such before the Society they would be carrying out one of the chief objects for which it was founded.

XI. *On some Facts connected with the Systems of Scientific Units of Measurement.* *By* T. H. Blakesley, *M.A.*, *M.I.C.E.*[*]

It seems to me a not unfitting opportunity, as a sequel to Prof. Rücker's suggestion, that we should modify some of our scientific formulæ, to inquire whether, with a change of formulation, we may not advantageously effect a change in some of the units themselves. That the C.G.S. and the practical system of units do not satisfy all the requirements of a perfect system of mechanical units must, I think, have occurred to others as well as to myself; and I mention this, not as an occasion for surprise, for indeed it would have been surprising if any system brought into vogue twenty years ago should still be found even to approach perfection, but rather in order to draw attention to the points in which these systems fail to give satisfaction, with a view to possibly rectifying their shortcomings.

The sense of imperfection may of course arise in the failure to attain to any one of the various ends which it should be the aim of a good system to reach.

I imagine that the following would be generally considered to be among those ends:—

First. All the quantities considered should be so connected by the equations representing the laws of nature, that no coefficients are required in expressing any units in terms of others. [Correlation.]

Secondly. Where quantities are recognized as being essentially of the same nature, they should be measured by the same unit. [Simplicity.]

Thirdly. Such a system should embrace all of the physical ideas which occur. [Comprehensiveness.]

Fourthly. Such a system should, so far as is compatible with other requirements, have its units closely and decimally connected with natural units. [Naturalness.]

Fifthly. The units should, subject to other considerations, agree with established though arbitrary units of measurement, actually or decimally. [Convenience.]

These five heads may be briefly described as:—

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[*] Read December 8, 1888.

Correlation.
Simplicity
Comprehensiveness.
Naturalness.
Convenience.

I lay no particular stress upon the order in which these ends should be placed, though in my opinion the above order seems the true one, and I think most people would agree in putting Correlation at the top of the list. Its importance is so great that it has in fact been more completely realized than any of the others, most physical ideas having their due formulæ assigned and recognized. It has been the more easy to do this, since the process is absolutely independent of the real units employed. The ideas need only to have their physical nature truly comprehended.

In the second point—Simplicity—there has been much less progress made. In electrical matters, for instance, though in describing a magnitude it may be perfectly well understood what units of length, mass, and time form the fundamental basis of the measurement, it is further necessary to state whether the Electrostatic or the Electromagnetic system is contemplated.

Again, heat is recognized as a form of energy, and yet the two are not referred to the same unit. If they were so, Joule's equivalent would always be unity. The defects in Comprehensiveness have been lately discussed in this Society by Prof. Rücker*. The one with which I have been brought most in contact is the omission of temperature from the systems in use. I think that if generalizations as great as the Electromagnetic Theory of Light are to be made by the help of systems of units, the Thermometer ought at once to be brought in out of the cold region of unsympathetic isolation and independence.

In chemical physics are to be found some notable omissions in atomic heat and electrochemical equivalence. When we examine the units to note how many are founded upon natural magnitudes, we recognize that the second is not a subdecimal part of the solar day, but only the $\frac{1}{86400}$ of that natural unit. To set against this, however, it is a unit which is in common use.

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* *Ante*, p. 37.

The centimetre is decimally connected with the earth's quadrant, and the unit of density is that of water, the gramme merely being derived from the centimetre and density of water by proper correlation.

In the quadrant-volt second, or practical system, the length unit is brought up to practical coincidence with the earth-quadrant; the second, as before, is the unit of time; and the other quantities are only decimally connected with the C.G.S. I am not aware that any accidental coincidences occur between other units of this system and natural ones, the nearest approach being probably the volt to the E.M.F. of a Daniell cell.

But it is when we compare the units with those of an arbitrary nature perhaps, but in common use, that the failure in actual coincidence or in decimal relation is most striking. The failure to introduce to any great extent the metrical measures of length into England as against the inch is a well-known case. Again, the horse-power and the erg per second are not decimally related.

So inconvenient is this last defect in the department of electromechanics, that Mr. Preece has suggested that engineers should give up the H.P. of James Watt and adopt in its stead 1000 watts. This suggestion has not, so far as I know, met with any support, perhaps because the modifications in the definition of the unit which would be necessary for its clear appreciation from a mechanical engineer's point of view have not been clearly stated. The evil implied in the suggestion is, however, great; and, if possible, something ought to be done to assuage the acute suffering which engineers feel in passing from H.P. to watts.

I think it will be conceded, too, that even the more hardened physicist feels some pangs of passage on the troubled sea of transformation from the *terra firma* of the Electrostatic system to that of the Electromagnetic; and I do not think a rapid answer could be given by many persons to the question, What is the value, in C.G.S. electrostatic units, of a current of (say) $33\cdot\dot{3}$ practical electromagnetic units? Yet in following out the experiments of Dr. Hertz, with which Mr. Tunzelmann has made us familiar, such a calculation must, I think, be made.

I will here give two formulæ, which will enable any magnitude given in one system to be brought into the other.

Let $k =$ the exponent of l in electromagnetic system,
$h =$ " " m " " "
$n =$ " " l in electrostatic system,
$q =$ " " m " " "
for any kind of magnitude brought into due correlation with others.

Then, to reduce between practical (quadrant-volt-second) units electromagnetic, and C.G.S. units electrostatic :

$$1 \text{ practical unit} = 3^{n-k}10^{10n-11q-k} \text{ electrostatic C.G.S.}$$

To reduce between C.G.S. electromagnetic and C.G.S. electrostatic :

$$1 \text{ C.G.S. electromagnetic unit} = 3^{n-k}10^{10(n-k)+11(h-q)} \text{ electrostatic C.G.S. units.}$$

The 3 in these equations is meant as a sufficient approximation to the number 2·998, which is one tenth of "v" in quadrants per second.

I have myself not seen such reduction-formulæ elsewhere, and under the present systems I think they may be found of use.

But it would be far better than employing such formulæ to have but one system for all measurements, and do away with this inconvenient bimetallism in the currency of science.

And in this case the way out of the difficulty is easy. We have only to adopt "v" as the unit of velocity. With this one restriction any magnitude will be expressed in a system based upon any units by the same number in either system. Such a system would be a veritable coalition, and for distinction might be so called.

A coalition system, then, merely requires that the unit of velocity shall be a certain one, which is also a natural one. It reduces the degrees of freedom in the choice of units by 1, and, without prejudice to Prof. Rücker's method of formulation, will allow us to fix almost any two other units arbitrarily. The two cannot of course be time and length, for velocity simply involves these alone. If the second were retained, the unit of length would be 30 quadrants. If the quadrant were retained, the unit of time would be $\frac{1}{30}$ of a second.

If it be urged that a velocity of 30 quadrants per second is a large one, I would reply that it is only 30 times the present unit of velocity in the practical system ; and that experience shows that extremely high or low units do not materially affect calculations. The unit of capacity is a million times the microfarad of our laboratories, and that of velocity about a million times the speed of an express-train. The use of mega- and micro- has now been found by experience to present no difficulty or objection.

Thus this change in the unit of velocity would not introduce inconvenience in itself ; but unfortunately, since $v=\frac{l}{t}$, its change would necessitate one indirectly in either length or time, and clocks and meters are so commonly in use that any interference in their present constructions would be felt a serious thing. Of the two changes, that of the time unit seems to me simplest. To take $\frac{1}{30}$ of a thing which itself is $\frac{1}{60}$ of a minute, that again being $\frac{1}{60}$ of an hour, does not appear so very radical an operation.

If we calculate the various units obtained by this change in the time unit and compare them with the quadrant-volt-second units, we shall find (column V.) that mass, length, and quantity of electricity remain unaltered ; time and conductivity become $\frac{1}{30}$; velocity, momentum, current, resistance, magnetic field, pole, and magnetic moment, 30 times ; force, acceleration, energy, and E.M.F., 900 ; and power, 27,000 times of the practical units ; while capacity sinks to $\frac{1}{900}$ of a farad.

This would, however, leave the connexion between the unit of power and 1 horse-power bad, for 1 unit would be about 36·2 H.P.

The correlation of temperature with other units would be, I imagine, best effected by the definition that a unit-of-temperature change shall be that through which a unit of energy in the form of heat would raise a unit of matter possessing unit specific heat ; in other words, the equation

$$ml^2t^{-2}=Jmc\theta$$

should be satisfied by $J=1$.

Since heat is recognized as energy, J must be numerical, notwithstanding arguments have been adduced against this view. It is sometimes said that J cannot be numerical because it changes with a change of units. This only arises from the fact that heat in the form $mc\theta$ is not properly correlated, because $(c\theta)$ is not so. Those who use this argument should apply it in some other case, when the fallacy would be apparent; *e. g.*

Power in watts $= 746$ horse-powers,

Power in ergs per sec. $= 10^7 \times 746$ horse-powers.

Here the factor has changed with a change of units, but no one would deny its being numerical, or the relation of one magnitude to another of the same nature.

If therefore J is reduced to unity by the above definition, we have $(c\theta) = l^2t^{-2}$ for the dimensions of temperature × specific heat; and if c is taken as numerical, these become the dimensions of θ.

As regards the relation between a degree so obtained and a thermometer-degree, we have the following facts to go upon:—

$4\cdot2 \times 10^7$ ergs raise 1 gram of water 1° C.,

$\therefore$ 1 erg raises 1 gram of water through $\left(\frac{1}{4\cdot2 \times 10^7}\right)^\circ$ C.

$\therefore$ 1° C. $= 4\cdot2 \times 10^7$ degrees C.G.S. (water).

1° F. $= 2\cdot\dot{3} \times 10^7$ " " "

1° R. $= 5\cdot25 \times 10^7$ " " "

These degrees therefore do not fit in decimally with the arbitrary scales in use.

But the number $4\cdot2$ may be written $\frac{1}{\cdot2381}$. If therefore we use as the standard of specific heat a substance which, referred to water, will have the specific heat ·2381, we shall then have

1° C. $= 10^7$ C.G.S. (substance in question).

Now Regnault's value for specific heat of air at constant pressure is ·2375, Wiedemann's value is ·2389. The mean of these is ·2382.

If therefore we adopt the specific heat of air at constant pressure as the standard of specific heat, we have

1° C. $= 10^7$ of 1 C.G.S. (air) unit.

The practical unit would be 10^{18} of a C.G.S. unit, and therefore would be 10^{11} of a degree Centigrade.

In relation to the thermometer, air has other well-known advantages over liquids, and is already the standard for specific inductive capacity and magnetic permeability.

This decimal relation of the Centigrade thermometer would not exist if the coalition of the Electromagnetic and Electrostatic systems were carried out on the lines I have indicated, viz. by making $\frac{1}{30}$ of a second the unit of time. The new unit of temperature would then be 900 times that of the electromagnetic quadrant-volt-second air unit.

Hence

1 unit of temperature $= 900 \times 10^{11}$ of 1 degree Centigrade.
„ „ $= 1620 \times 10^{11}$ „ Fahrenheit.
„ „ $= 720 \times 10^{11}$ „ Réaumur.

The endeavour to bring the horse-power and the unit of power into decimal relation may be guided by throwing the formulæ into terms of length, time, and power ; and I have accordingly given a table of the ideas involved on this basis, in column VII. Any magnitude which appears in these formulæ unencumbered by the symbol of power will of course be unaltered by a mere change in its unit. Hence, if we adopted as the unit of power, ·746 of the watt or a decimal multiple of that quantity of power, we could bring the unit of power into decimal relation with the horse-power without altering the units in the practical system of time, length, velocity, acceleration, resistance and conductivity, and capacity. Thus the expensive standards of our laboratories, of resistance and capacity, our resistance-boxes and our microfarads, would remain to us. But our boxes of grammes would fall in value. The unit of temperature, however, would not be altered, so that Centigrade thermometers would remain. Column VIII. expresses the results of such a change.

No doubt if we did not suffer from an hereditary duodecimal taint, we might discard the $\frac{1}{86400}$ of a day, called a second, and adopt the $\frac{1}{100000}$ of a day as our unit of time, and that interval would be ·864 of a second, which number happens to be the square root of ·746; and this would give us a coincidence in magnetic field, whose formula is $v^{-\frac{3}{2}} t^{-1} p^{\frac{1}{2}}$. But magnetic field is not a magnitude of which we keep instrumental standards, as we are wont to do of resistance, capacity, mass, time, length, and sometimes of E.M.F.

It will be observed that in the formulæ the symbol of power occurs either simply or raised to the power of $\frac{1}{2}$, and we may bring power into decimal relation with horse-power either by taking ·746 of the unit or 7·46. The square roots of these relations are ·86373 and 2·73136. Using the latter number we should obtain no further coincidences among the magnitudes.

In reviewing the results indicated by the above calculations, in dealing with the units of science in such a way as to obtain :—

(1) The correlation of the thermometer ;

(2) The unity of the two systems, Electrostatic and Electromagnetic;

(3) The bringing of the unit of power into decimal relation with the horse-power ;

I come to the conclusion:—

(1) The thermometer Centigrade may be brought into decimal relation with temperature-units on the C.G.S. system or quadrant-volt-second system by adopting the specific heat of air at constant pressure as unity.

(2) That such decimal relation is compatible with a change in the unit of power to $\frac{1}{1000}$ of 1 H.P., which latter change could take place without changes in the units of time, length, velocity, resistance, and capacity, but would involve a change in the important standards of mass.

(3) That the union of the two electrical systems could be brought about in the simplest way by taking $\frac{1}{30}$ of one second as the unit of time ; such a change retaining the gramme-boxes, and the metre decimally, and the coulomb, but requiring the unit of capacity to be $\frac{1}{900}$ of 1 farad and the unit of resistance to be 30 ohms.

To my mind the last change recommends itself. No real change would be necessary in the face of the chronometer, or in the metre, or in the gramme-boxes. Each microfarad would have to be called ·0009, and each second 30, and every

Mechanical and Electromechanical Units.

I. Electrostatic formula in l, t, m.	II. Idea.	III. Electromagnetic formula in l, t, m.	IV. Practical Units in terms of C.G.S. Electromagnetic.	V. Coalition Units. Quadrant, $\frac{1}{30}''$, $\frac{\text{gramme}}{10^{11}}$ in terms of Practical Units.	VI. Coalition Units, as in V., in terms of C.G.S. Electrostatic Units.	VII. Electromagnetic formula in l, t, and p (power).	VIII. Electromagnetic Units. Quadrant, Second, ·746 Watt, in terms of Practical Units.	IX. Electromagnetic Units. Quadrant, Second, 7·46 Watts, in terms of Practical Units.	X. Coalition system. Quadrant, $\frac{1}{30}''$, ·746 Watt, in terms of Practical Units.
	Time.	t	1	$\frac{1}{30}$		t	1	1	$\frac{1}{30}$
	Mass.	m	10^{-11}	1		$l^{-2}\ t^{3}\ p$	·746	7·46	·0000276
	Length.	l	10^{9}	1		l	1	1	1
	Velocity.	$l\ t^{-1}$	10^{9}	30		$l\ t^{-1}$	1	1	30
	Force.	$l\ t^{-2}\ m$	10^{-2}	900		$l^{-1}\ t\ p$	·746	7·46	·02487
	Acceleration.	$l\ t^{-2}$	10^{9}	900		$l\ t^{-2}$	1	1	900
	Momentum.	$l\ t^{-1}\ m$	10^{-2}	30		$l^{-1}\ t^{2}\ p$	·746	7·46	·008289
	Energy.	$l^{2}\ t^{-2}\ m$	10^{7}	900		$t\ p$	·746	7·46	·02487
	Power.	$l^{2}\ t^{-3}\ m$	10^{7}	27000		p	·746	7·46	·746
$l^{\frac{1}{2}}\ t^{-1}\ m^{\frac{1}{2}}$	E.M.F.	$l^{\frac{3}{2}}\ t^{-2}\ m^{\frac{1}{2}}$	10^{8}	900	3	$l^{\frac{1}{2}}\ t^{-\frac{1}{2}}\ p^{\frac{1}{2}}$	·864	2·731	4·731
$l^{\frac{3}{2}}\ t^{-2}\ m^{\frac{1}{2}}$	Current.	$l^{\frac{1}{2}}\ t^{-1}\ m^{\frac{1}{2}}$	10^{-1}	30	9×10^{10}	$l^{-\frac{1}{2}}\ t^{\frac{1}{2}}\ p^{\frac{1}{2}}$	·864	2·731	·1577
$l^{-1}\ t$	Resistance.	$l\ t^{-1}$	10^{9}	30	$\frac{1}{30}10^{-9}$	$l\ t^{-1}$	1	1	30
$l\ t^{-1}$	Conductivity.	$l^{-1}\ t$	10^{-9}	$\frac{1}{30}$	30×10^{9}	$l^{-1}\ t$	1	1	$\frac{1}{30}$
$l^{\frac{3}{2}}\ t^{-1}\ m^{\frac{1}{2}}$	Quantity.	$l^{\frac{1}{2}}\ m^{\frac{1}{2}}$	10^{-1}	1	3×10^{9}	$l^{-\frac{1}{2}}\ t^{\frac{3}{2}}\ p^{\frac{1}{2}}$	·864	2·731	·00526
l	Capacity.	$l^{-1}\ t^{2}$	10^{-9}	$\frac{1}{900}$	10^{9}	$l^{-1}\ t^{2}$	1	1	$\frac{1}{900}$
$l^{\frac{1}{2}}\ t^{-2}\ m^{\frac{1}{2}}$	Magnetic field.	$l^{-\frac{1}{2}}\ t^{-1}\ m^{\frac{1}{2}}$	10^{-10}	30	90	$l^{-\frac{3}{2}}\ t^{\frac{1}{2}}\ p^{\frac{1}{2}}$	·864	2·731	·1577
$l^{\frac{1}{2}}\ m^{\frac{1}{2}}$	Pole.	$l^{\frac{3}{2}}\ t^{-1}\ m^{\frac{1}{2}}$	10^{8}	30	10^{-1}	$l^{\frac{1}{2}}\ t^{\frac{1}{2}}\ p^{\frac{1}{2}}$	·864	2·731	·1577
$l^{\frac{3}{2}}\ m^{\frac{1}{2}}$	Magnetic moment.	$l^{\frac{5}{2}}\ t^{-1}\ m^{\frac{1}{2}}$	10^{17}	30	10^{8}	$l^{\frac{3}{2}}\ t^{\frac{1}{2}}\ p^{\frac{1}{2}}$	·864	2·731	·1577
	Temperature.	$l^{2}\ t^{-2}$	10^{18}	900		$l^{2}\ t^{-2}$	1	1	900

30 ohms 1 unit. A habit has arisen of attaching particular names to units. This, without doing very much good, adds greatly to the stability of the unit, and makes any subsequent change so much the more difficult. The practice has almost always been confined to electricians, somewhat too oblivious of the mechanical basis of a proper system.

Sir William Thomson, in a paper on units of measurement, contemplates a time when many of the units we now employ will become things of the past by the introduction of the universal gravitation method of connecting mass with length and force.

The naming of particular units employed for a time in the process of this development cannot but retard its progress, and the unity of science should take place independently of the subdivisions which may exist to-day.

It is highly probable that chemical science may soon require a modification of our present units, and we must not keep combining weights, atomic heat, and so on, uncorrelated with the rest of the body of science because the development of science has taken the order it has done. It might have been quite otherwise.

Dr. Whewell entertained a speculation as to whether the laws of motion would ever have been discovered by an intellectual race of jelly-fish inhabiting a world in which were no solid bodies.

In like manner we may imagine that had Torpedoes, or other animals armed with a controllable electrical equipment, won the race for supremacy against man, the laws of electricity might have been reached before the mechanical laws. Faraday and Maxwell would have preceded Galileo and Newton, and Gymnotus might have exhibited in submarine air-cases that uncanny biped Homo who could give you a curious and mysterious shock with a stick or a stone.

Prof. Rücker expressed his opinion that temperature would be best correlated by expressing it as energy, instead of making its dimensions $l^2 k^{-2}$ as suggested in the paper.

Dr. S. P. Thompson thanked the Author for his prefix "*megisto*," denoting "multiply by 10^{12}." Referring to the choice of air as the standard for specific heat, he remarked that convenient coincidences were not always to be trusted, and that the specific heat of air depended on the pressure to which it was subjected.

XII. *On a Method of Comparing very unequal Capacities.*

By Mr. H. Fison, *D.Sc.**

(Abstract.)

One coating of each condenser is joined to earth and to one end A of a high resistance (20,000 or 30,000 ohms) through which a current is flowing. The small condenser is charged to the P.D. existing between the ends A, B of the resistance and discharged into the large one. This is repeated a great number of times. If C be a point between A and B, the resistance between A and C may be varied until the P.D. between them is equal to that between the coatings of the condensers after *n* operations. If the insulated coatings be now joined to C through a galvanometer no deflexion will result. The relation between the capacities C_1 and C_2 of the large and small condensers is given by

$$\left(1+\frac{C_2}{C_1}\right)^m=\frac{R_{AB}}{R_{BC}},$$

where R_{AB}, R_{BC} are the resistances between AB and BC respectively.

Since time is required to prepare the operations the instantaneous capacities cannot be compared, and accordingly the measurements are taken after a definite time of electrification. A special rotating key was shown for performing ten operations per revolution, in which a trigger arrangement was provided for stopping the rotation after a predetermined integral number of revolutions. The method has been used for comparing a small air-condenser with a microfarad. The capacity of the former was also calculated electrostatically (correction being made for the edges), and that of the latter measured electro-magnetically by a ballistic galvanometer.

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* Read June 9, 1888.

The results give a value for v equal to $2{\cdot}965 \times 10^{10}$. In these experiments the capacity of the rotating key was allowed for. Under favourable conditions, capacities in the ratio of 1 to 1000 or 1 to 10,000 can be compared with an accuracy of $\frac{1}{4}$ per cent.

Prof. Ayrton thought the novelty of the arrangement was in the rotating key, as the method of comparing unequal capacities by charging the smaller and discharging it into the larger a considerable number of times had been described and used by himself and Prof. Perry in their experiments on the Specific Inductive Capacity of Gases.

XIII. *On the Divergence of Electromotive Forces from Thermochemical Data.*

By E. F. HERROUN, *Professor of Natural Philosophy in Queen's College, and Demonstrator in King's College, London*[*].

THE fact that with many voltaic cells the electromotive forces observed experimentally do not accord with values calculated from the thermochemical equations representing the reactions occurring within the cells, has been the subject of remark by several investigators, including Favre, Julius Thomsen, Braun, Helmholtz, Willard Gibbs, Wright and Thompson, and others. But although, at the present time, we are in possession of a knowledge of the actual and computed values of the electromotive forces of a large number of voltaic cells, anything approaching a complete explanation of the reason why certain cells should give electromotive forces in excess, others in defect, others again according almost exactly with the calculable values, is still wanting.

It was in the hope of deciding certain questions and of going some way on the road to a final solution of the problem that the present research was undertaken.

Certain cells, like the 'Grove' or 'Bunsen,' in which nitric acid is reduced by the evolved hydrogen, are said to furnish, with nitric acid of certain degrees of concentration, electromotive forces somewhat in excess of the values calculated from the thermal effects accompanying the chemical changes;

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[*] Read January 26, 1889.

but the effect of solution of the oxides of nitrogen or their evolution as gas, together with other effects of dilution of the acid, complicate the subject from the theoretical standpoint, and their non-reversibility renders them unsuited for experiment. Cells of the Daniell type are therefore greatly to be preferred, both for theoretical consideration and direct experiment.

Thanks to the careful and accurate data furnished by the published researches of Dr. C. R. A. Wright and Mr. C. Thompson, and their clearly tabulated results[*], one can see at a glance whether the electromotive force furnished by two metals immersed in solutions of their corresponding salts accords with, exceeds, or falls below the value calculated from thermochemical data, and the exact amount of the discrepancy.

The explanations which might be given of the cause of the departure of observed E.M.F.s from the calculated values in different cells may be classified under the following heads:—

1. Certain cells in which the anticipated chemical change does not occur, but some other, different, reaction which evolves heat, to an extent which would account for the observed E.M.F. This is brought about by the special behaviour of a metal surface, and probably depends upon—

2. The coating of a metal surface by films of oxide or subsalts either more or less readily reduced than the normal salt of that metal.

3. The effect of dissolved gases, notably oxygen, in the solutions of the metallic salts.

4. The hydration or solution of the salts formed in some cases, but not in all, supplementing or diminishing the E.M.F. due to the reaction of the solid salts.

5. The evolution or absorption of sensible heat in the cell, and the loss or gain in electric energy, corresponding thereto.

In illustration of case 1 may be mentioned a cell consisting of aluminium in aluminic sulphate opposed to zinc in zinc sulphate, in which the thermochemical data would indicate that aluminium would be the metal attacked, giving, when opposed to zinc, an E.M.F. of about 1 volt; whereas, in practice, zinc is the metal which is attacked, and the cell furnishes an E.M.F. of about ·53 volt. But we know that the chemical reaction $Al_2(SO_4)_3+Zn_3=3(ZnSO_4)+Al_2$ does not occur, so that the aluminium plate merely acts as an imperfect substitute for an entirely non-attackable substance such as platinum or carbon, and having *hydrogen*, not aluminium, deposited on its surface.

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[*] "The Determination of Chemical Affinity in terms of Electromotive Force," pt. ix., Proc. Phys. Soc. vol. vi. pt. iv., and Phil. Mag. ser. 5, vol. xix. No. 118.

That this is the case, and that the reason is probably to be found in the superficial coating of the aluminium plate by an oxide or suboxide, has been already put forward and received experimental verification by Dr. A. P. Laurie*, who has investigated the effect of amalgamating the aluminium, which better enables it to come in actual contact with the liquid and causes the E.M.F. of the cell to actually reverse in direction.

The heat developed by the oxidation or conversion into sulphate of the zinc with the liberation of hydrogen would be more than sufficient to account for the observed electromotive force; hence if it be desired to trace any connexion between the absorption or evolution of heat and the anomalous E.M.F. of any cell, it is the actual chemical change that does occur which must be made the basis of the calculation. Further, the term "*Thermovoltaic Constant*" (employed by Messrs. Wright and Thompson to denote the extent of departure of the E.M.F. of a cell from thermochemical data; zinc being taken as the standard from which the voltage is reckoned) in cases such as aluminium can merely express the fact that a plate of aluminium in its compact form, and probably covered by its protecting film of oxide, is less actively attacked than it ought to be from theoretical consideration of the heat of formation of its salts; but it must not be accepted as denoting any occult power of transforming electric energy into heat or *vice versâ*.

From purely *à priori* considerations one would expect that metals like aluminium and magnesium, which in the amalgamated or finely divided condition readily decompose water, would, in the compact form, become covered superficially with oxide and behave to a greater or less extent as a hydrogen plate. It is therefore largely to this cause that one would look for an explanation of the large +values assigned to the "thermovoltaic constants" of aluminium, magnesium, and possibly iron. The observation by Wright and Thompson, that the substitution of dilute sulphuric acid for ferrous sulphate as the liquid surrounding the iron plate of an iron-copper cell uniformly tends to *lower* the E.M.F., which effect they refer to "local action," distinctly supports the view that even with iron there is a tendency to decompose water and coat itself with hydrogen, and thus acquire a higher relative potential.

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* Phil. Mag. ser. 5, vol. xxii. p. 213.

A further consideration of case 2 will be met with later in this paper. I will merely mention that a considerable difference in E.M.F. due to the superficial coating of a copper plate with suboxide has been already noticed in the use of standard Daniell cells by Dr. J. A. Fleming[*] and myself[†], and that what is rare and comparatively unimportant in the case of copper may become the rule and of greater importance in some other combinations.

In case 3 the nature of the effects of dissolved oxygen in oxidizing a metallic surface or in removing traces of electrolytic hydrogen and its depolarizing effect in the latter case are too obvious to need much comment; only I would draw attention to the fact that by its action the electromotive force on open circuit, as measured by an electrometer, or with a balance of difference of potential, as in Latimer Clark's method, or even when measured by a galvanometer of many thousand ohms resistance, is liable to be in excess of that due to the supposed chemical reaction, and therefore to give false ideas as to the energy-producing power of the cell.

Case 4 has been suggested as a very probable source of divergence, since (1) in cells in which a very sparingly soluble salt is employed, the solution, especially in the vicinity of the electrode, may become saturated, and if any more salt be formed it will necessarily be produced in the solid form, which may, or may not, be dissolved in the rest of the liquid; (2) it is difficult to see how the tendency of a salt to combine molecularly with water to form a hydrate or to dissolve as a solution, can directly affect the combination of the metal with the acid radical to form that salt, and therefore we may be justified in concluding that both the total heat evolved and the E.M.F. of a cell are due to the algebraic sum of effects of events which take place successively. But since the water in different solutions is differently combined or related with the anhydrous salt, it appears possible that the energy which in some cases is transformable into electric energy, or, to use Helmholtz's suggestive names, is "free," in other cases may be simply "bound energy."

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[*] Phil. Mag. ser. 5, vol. xx. p. 132; Proc. Phys. Soc. vol. vii. p. 168.

[†] Phil. Mag. ser. 5, vol. xxi. p. 12; Proc. Phys. Soc. vol. vii. p. 276.

The main interest of the question, however, centres in case 5, since many physicists seem to have tacitly assumed that the explanation of the divergence of actual E.M.F.s from the computed values is simply that with cells giving a deficiency of electromotive force a portion of the chemical energy appears as heat in the cell, while cells giving an excessive electromotive force are spoken of as doing so "at the expense of sensible heat." They have not, however, given any reason for such differences of action, and they do not appear to have considered all the consequences which the adoption of the latter part of this view would entail.

Helmholtz, in his papers "Zur Thermodynamik chemischer Vorgänge,"[*] suggests the treatment of a voltaic cell as a reversible thermodynamical process, and that the nature of its working may be detected by the difference of E.M.F. brought about by change of temperature. The cell selected for examination is his zinc-mercurous chloride battery, the E.M.F. of which is very slightly increased by rise of temperature; and the chief discussion is the relation of the vapour-tension of the zinc-chloride solution, the "free energy of the salts," and the electromotive force dependent on differences of concentration.

But that part of his conclusion in which he states that "*the electromotive force between the metals increases with heating*, i.e. *the calomel battery belongs, as I have already mentioned, to the batteries which fix heat, which in part work at the expense of the thermometric heat of surrounding bodies*," is based either purely on thermochemical data or without due consideration of what the adoption of that view would entail in the case of the strictly comparable zinc-mercurous sulphate cell of Latimer Clark, which decreases in E.M.F. by a considerable proportion as its temperature is raised, although this also is a cell giving an E.M.F. in excess of the supposed thermochemical effect.

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[*] *Sitzungsbericht. d. Akad. d. Wissenschaft. zu Berlin.* Translated and pub. in Physical Memoirs, by the Phys. Society, vol. i. pt. 1.

Now viewing a cell as a strictly reversible thermic engine, if a portion of its E.M.F. be due to the absorption of sensible heat, then, when worked backwards, an exactly equivalent amount of heat would be evolved in the cell. Therefore, taking two Latimer Clark cells, joined by their like poles, one at a high, the other at a low relative temperature, the one at the low temperature having the higher E.M.F. will send a current against the E.M.F. of the one at the higher temperature.

But the current through the cold one will be direct, while that through the warm one will be inverse, so that heat will be absorbed in the cell at low temperature and evolved in the cell at high temperature; and since for unit quantity of electricity flowing the same amount of chemical work will be performed in each cell, but reversed in one, we should have an instance of a thermodynamical arrangement working by means of a difference of temperature, the effect of which would be to cool the cold cell and heat the hot one; or heat would be transferred from the cold to the hot body, and be made capable of performing mechanical work, instead of requiring its expenditure. It does not appear to me that this could be adequately explained by the different heats of dilution of the salt at different temperatures, so that the result of believing that a Latimer Clark cell works partly "at the expense of sensible heat" is a direct contradiction of the principle of the second law of thermodynamics, as this would require that its E.M.F. should rise with rise of temperature. That it is the postulate with regard to mercury cells, and not the second law, which is fallacious, I hope to show later in this paper.

If two plates of zinc, one amalgamated and the other plain, be immersed in a solution of zinc sulphate, on completing the external circuit a current is found to flow from the amalgamated to the non-amalgamated plate through the cell;

but Prof. J. Willard Gibbs in his paper "On the Equilibrium of Heterogeneous Substances,"* remarks that such a current could not have the effect of dissolving zinc from the amalgamated plate and depositing it on the non-amalgamated plate, as this deposited zinc might be immediately redissolved in mercury to form an amalgam with merely the absorption of sensible heat.

By the use of pure zinc foil in solutions of zinc sulphate, free from oxygen, I have found that the difference of potential between amalgamated and plain zinc becomes exceedingly small; and in the case of a cell freely exposed to air the current on a circuit of only 1·5 ohm's resistance did not deposit any weighable quantity of zinc on the plain zinc electrode after passing for seven days. This, therefore, is in confirmation of the theoretical conclusion from the second law put forward by Willard Gibbs, and renders it more imperative to stringently examine any reputed case of the production of electrical energy at the expense of thermometric heat.

It therefore became important to examine on an experimental basis the theory that reversible heat effects are associated with cells giving anomalous electromotive forces.

Method.

The method usually adopted may be briefly described as follows:—The experimental cell was constructed of a thin glass beaker as the outer vessel, containing one of the metal electrodes in a solution of one of its salts, and a porous earthenware cell, which contained the other metal in a solution of its corresponding salt. Cells of the Raoult form could not be employed on account of their inconvenience for thermal measurements and their necessarily high internal resistance. Attempts to reduce resistance by substituting animal membrane, parchment-paper &c. for the porous cell were abandoned owing to the facility with which they allowed small but significant quantities of the solutions to mingle by diffusion, and so permit the deposition of one metal on the more electropositive one, generating local heat and altering the electrical constants. The cell was supported by the rim of the beaker resting on a cork edge which was fitted into the top of a tin cylinder, the latter being itself suspended within a larger cylinder of bright tin. With this apparatus, when containing the usual working quantities of solutions, the rate of loss of heat was equal to $\frac{1}{100}$ of a gram-degree Centigrade for each minute-degree above the surrounding air temperature, for the few degrees of the experimental range.

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* Trans. Connect. Acad. vol. iii. pts. 1 and 2.

Thermometers graduated to fractions of a degree were inserted, one in the inner porous cell and another in the outer cell; and in cases in which a slight inequality of heating at the two electrodes occurred, a mean of the rise of temperature of the two thermometers was taken as best expressing the net heat-evolution.

The external battery, used for sending currents against the E.M.F. of the experimental cell, consisted of either a Daniell cell or a form of constant bichromate battery, in accordance with the E.M.F. of the experimental cell to be overcome. The current was measured by a calibrated galvanometer, which gave a deflexion of 47°·5 with $\frac{1}{10}$ of an ampere, and was regulated by the introduction of the necessary resistance.

It was first proved in the case of a cell, cadmium-copper sulphate, which gives an E.M.F. almost exactly in accordance with its calculated value, that the heat which is generated by a current passing through the cell depends only upon its resistance, and is independent of the direction in which that current passes.

This being so, if the permanent experimental value of the E.M.F. of any cell differs by e volts from its calculated value ($E - E_h = \mp e$), an amount of heat equal to $\frac{ect}{J}$ ought to be evolved or absorbed when a current of strength c passes for t seconds through the cell.

Thus if the cell be one giving an excess, or $E - E_h = +e$, there should be an absorption of heat when the cell is worked forwards and an evolution when worked backwards against the E.M.F. of the cell. Exactly the reverse would hold with cells giving a deficit from the theoretical value.

Since the heat due to resistance will be evolved in both cases, we have for the net heat evolved

$$H=\frac{c^2 r t \mp e c t}{J};$$

and where the value of $e c t$ is negative we have a negative total if it exceed the value of $c^2 r t$.

In order therefore that the *absorption* of heat may be a maximum, with cells giving an excess of E.M.F. when worked forwards, or with cells giving a deficiency when worked backwards, the current-strength must be adjusted in accordance with the determined value of the internal resistance of the particular cell.

Since the heat absorbed will be

$$-H_1=\frac{e c t-c^2 r t}{J},$$

or

$$-H_1 \propto e c-c^2 r,$$

and

$$\frac{dp}{dc}=e-2 c r;$$

equating to maximum value we have

$$c=\frac{e}{2r}$$

as the current-strength giving the greatest absorption of heat in the case of a cell in which the computed and observed E.M.F.s differ by e volts and internal resistance is r ohms.

It is thus a matter of importance to determine the internal resistance, not only in order to be able to calculate the heat generated against it, but also to calculate the best current-strength for observing the effects. In many cells, however, the resistance is so unavoidably high that c would be too small for any thermal changes to be detected ; and in other cases the current has to be increased above the best theoretical to a best practical strength, *time* being an important factor in such delicate thermal measurement.

It was found experimentally that the resistance in many cells is subject to considerable and, in some cases, enormous variations when currents are passing through the cell, being partly dependent upon the direction of the current. This forms one of the most serious difficulties in the experimental work, for if the resistance change with change of direction of the current, the difference of heating-effects might be due, at least in certain cases, to such change rather than to the value of $E-E_h$.

This difficulty was endeavoured to be met by using a high resistance galvanometer on a circuit of several thousand ohms as a measurer of the difference of potential between the poles of the experimental cell, when on open circuit and when direct or inverse currents are passing through it.

Taking E as the E.M.F. of the external battery and R its resistance, and $E_{,}$ the E.M.F. of the experimental cell, and $r_{,}$ its resistance, and letting $r_{,,}$ denote the total external resistance, including that of the current-galvanometer, we get for the difference of potentials between the poles of the experimental cell:

$$V=\frac{E_{,}(R+r_{,,})-E\,r_{,}}{R+r_{,}+r_{,,}} \text{ with direct currents,}$$

and

$$V_{,}=\frac{E_{,}(R+r_{,,})+E\,r_{,}}{R+r_{,}+r_{,,}} \text{ with inverse currents.}$$

Since, however, the value of $E_{,}$ (or indeed E) is subject to possible variation due to polarization, the above method fails to distinguish between change of difference of potential due to change of resistance and that due to polarization; and although the latter, if present, may be usually detected by suddenly interrupting the current and noting the value of $E_{,,}$, the difficulty still remains to some extent and is liable to vitiate the results.

From the causes just indicated it will be readily seen that extremely few cells are suited for thermal experiments when currents of moderate strength have to be sent through them in alternate directions. Consequently a large number of voltaic combinations were tried and abandoned as unsatisfactory, for some of the following reasons:—Certain cells are found to give marked variation in their internal resistance with different direction of current, commonly owing to the sparing solubility of at least one of the salts formed and the consequent coating of one or both plates with insoluble and badly conducting films (*ex. gr.* cells involving the sulphate, chloride, or bromide of lead, mercury, or silver, three typically exceptional metals). In other cases cells are rendered useless by the imperfect reversibility of the chemical change; as in cases in which it is attempted to deposit a metal which can replace hydrogen from water at all readily (*ex. gr.* magnesium, aluminium, zinc, and iron), hydrogen is liable, in some cases certain, to be liberated in place of the equivalent of metal. In other cells, such as lead or silver in contact with their sulphates or free sulphuric acid, on making either of those metals the anode in the cell, a peroxide (PbO_2 or AgO) is apt to form in spots on the surface of the metal plate and entirely alter both the E.M.F. and resistance of the cell, besides obviously altering the chemical work of electrolysis.

The desiderata in a cell intended for this work—namely tolerable freedom from polarization, perfect reversibility, low and constant internal resistance—are only even approximately fulfilled by a very few of a large number of cells examined by me, and therefore the experimental results are greatly restricted.

Mercury.

After dismissing aluminium and magnesium as being unsuitable, for reasons already stated, the metal which has the next highest "thermovoltaic constant" positive in sign is mercury, and being the metal which is replaced by most other metals, as copper, zinc, &c., it yields electromotive forces considerably in excess of the values deduced from the heat of formation of its salts. The insolubility and sparing solubility of the mercurous chloride, bromide, and sulphate, and the inadmissibility of mercuric salts, owing to their reduction in contact with metallic mercury, rendered it necessary to employ mercurous nitrate, and, accordingly, a copper-mercury nitrate cell was prepared, in which each metal was in a strong solution of its own nitrate, with a small proportion of free nitric acid, which is necessary to keep the mercurous salt in solution by preventing the formation of basic salts, and also diminishes the internal resistance. I found this cell gave an E.M.F. of ·43 volt, while from the thermochemical numbers of Julius Thomsen, viz.

$$[Cu, O, N_2O_5 Aq] = 52410 \text{ and } [Hg_2, O, N_2O_5 Aq] = 47990,$$

one would deduce an electromotive force of only ·095 volt.

(Wright and Thompson, adopting the older standards of measurement, give ·433 volt for the experimental and ·097 volt for the calculated value.)

When a current of ·2 ampere was passed in alternate directions through the cell, it was found that if the current was not continued for too long a period in one direction the resistance was moderately constant, though by the prolonged action of the current the solution became either nearly deprived of mercury, or saturated with mercurous nitrate in the layers near the metallic mercury, depositing crystalline nitrate on it and greatly increasing the resistance. But by allowing the current to pass for not more than 10 to 20 minutes in one direction this was avoided, and the rise of temperature which was observed corresponded very nearly with the heat due to $c^2 r t$, and was independent of the direction in which the current passed; showing therefore no absorption of heat with direct currents, as might be anticipated from its large excess of E.M.F., viz. ·335 volt.

This result was so plainly at variance with what was to be expected, that a solution of mercurous nitrate was electrolysed, using a platinum anode; but in this case also there was no indication of more chemical work being performed by the current than supplied by it, *i.e.* no absorption of heat was detected, but an evolution corresponding closely with the amount calculated from the values of $\frac{c^2 r t}{J}$. Hence the result of these experiments clearly pointed to the conclusion that mercury, if it be anomalous in its E.M.F., is not so on account of any transformation of sensible heat into electric energy; and I was therefore led to examine the thermochemical data of mercury upon which the calculated E.M.F.s have been founded.

Thermochemical Data of Mercury.

In the first place Julius Thomsen* appears to be the only experimentalist who has made determinations of the heats of formation of mercury salts, and since he depends upon the heat of formation of mercurous nitrate for the thermal values of almost all the other salts, if that were in error all the values derived from it would evidently be equally inaccurate. But the method he employed for the determination of the heat of [Hg_2, O, N_2O_5 Aq] does not appear to me entirely free from objection. He determined it by reducing the mercury from a solution of its nitrate by sulphur dioxide, and determined the heat of neutralization of [Hg_2O,2HNO_3Aq] by decomposing the nitrate with sodic hydrate; finding, as above stated, 47990 calories for the heat of formation of the nitrate in aqueous solution. Since, however, mercurous nitrate requires the presence of free nitric acid to maintain it in solution, he used a solution containing free nitric acid to an extent variously stated as 1·079 and 3·079 HNO_3 per molecule of $Hg_2(NO_3)_2$; as the latter number is mentioned twice, probably the former is a misprint. But the exact reaction which occurs between mercurous nitrate, free nitric acid, and SO_2 one may reasonably expect to be less simple than that required for exact thermal measurement.

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* *Thermochemische Untersuch.* Band iii. S. 355.

I have accordingly determined the heat of formation of mercurous nitrate in acid solution, by decomposing it with zinc amalgam, yielding zinc nitrate and metallic mercury; the results obtained are subjoined.

Mercury used.	Heat-units cooled.	Heat of Reaction.
1·362 grms.	265·5 grms ° C.	77,975
1·362 „	206·2 „	78,180
2·58 „	492·1 „	76,300
		Mean = 77,485

Since Thomsen finds 102,510 for [Zn, O, N_2O_5 Aq], the heat of formation of

$$[Hg_2, O, N_2O_5\,Aq] = 102{,}510 - 77{,}485 = 25{,}025 \text{ calories.}$$

If this be approximately true, it follows that the values given by Thomsen for the heats of formation of the chloride, bromide, and iodide of mercury must be greatly in excess of the true value. The heat of formation of [Hg_2, Cl_2] is given by him as 82,550, taking 47,990 as the value for the nitrate. But it is well known that calomel is reduced to metallic mercury by an acid solution of stannous chloride, although the heat evolved in the conversion of $SnCl_2$Aq into $SnCl_4$Aq is only 76,030. Hence splitting up Hg_2Cl_2 with the formation of stannic from stannous chloride would give, according to Thomsen, the following thermal equation :—

$$[\mathrm{Sn\,Cl_2, Cl_2, Aq}] - [\mathrm{Hg_2, Cl_2}]$$
$$76{,}030 \quad - \quad 82{,}550 \quad = -6520,$$

so that this reaction would be markedly endothermic and could not well occur at ordinary temperatures.

The matter, however, was put to experimental test, and it was found that although the reaction was slow, requiring about three hours for its completion at a temperature of about 15° C., still heat was continuously *evolved*, and, after calculating for loss of heat, gave the following values :—

Weight of Hg_2Cl_2.	Heat evolved.	Heat of Reaction.
5·067 grms.	120·76 grms.° C.	11,225
4·783 „	104·8 „	10,325
		Mean = 10,775

Giving 76,030 − 10,775 = 65,255 calories as the heat of formation of Hg_2Cl_2.

The electromotive forces calculated from these thermal data give for zinc-mercury nitrate cells $E_h = 1·673$ volt, the experimental value being 1·5 volt; and for zinc-mercury chloride cells $E_h = 1·028$, the experimental value being 1·043 (Helmholtz), or from 1·123 to ·988 (Wright and Thompson). Hence it is evident that mercury cells do not evolve electromotive forces of about half a volt in excess of their calculated values, the various statements to that effect being based on inaccurate thermochemical data. On the contrary, it will be noticed that the value of E_h for the nitrate cell is greater than the experimental value E, which may be due to an error in my determination of the heat of formation of mercurous nitrate, but more probably to the same cause acting as in the cases of lead and silver, both of which metals have negative "thermovoltaic constants," and which greatly resemble mercury in many chemical characteristics.

Silver.

The "thermovoltaic constants" for silver, given by Wright and Thompson, vary from ·395, in the case of the nitrate, to ·02, in the case of the iodide, but are all negative in sign. It is noticeable that the larger values are attached to the soluble salts, while the insoluble chloride, bromide, and iodide have much smaller "constants," and these depend largely on the nature of the liquid in which the haloid salt is suspended; thus AgCl in solution of $ZnCl_2$ has a thermovoltaic constant of ·112 to ·062 volt, while a copper-silver chloride cell in which the AgCl is suspended in $CuCl_2$ solution has a difference of only ·001 volt from the computed value. The causes producing the large divergence in the case of the nitrate or sulphate are, therefore, probably different in nature from those producing the small and very variable "constants" (?) in the case of the chloride, bromide, or iodide.

In order to test whether a cell consisting of silver in a solution of its nitrate, opposed to some other metal, gives rise to reversible heat-effects or not, a copper-silver nitrate cell was prepared and used in the manner previously described. It was found that when small currents (·1 to ·2 amp.) were sent in alternate directions through the cell by means of another battery, apart from the heat due to the square of the current and the resistance, there was an evolution of heat when the cell was worked forwards, and an absorption of heat when worked backwards so as to dissolve silver and deposit copper.

The following numbers, taken from one set of experiments, show the difference of heating with synergetic and opposed currents (E.M.F. of the cell ·431 to ·428 volt):—

			Mean rise of Temp.
Direct current of ·2 ampere for 20 minutes			0°·6 C.
Adverse current	"	"	0·1 C.
Direct current	"	"	0·35 C.
Adverse current	"	"	0·00 C.
Direct current	"	"	0·45 C.
Adverse current	"	"	0·00 C.

Mean rise with direct currents =0°·466 C.
Mean rise with adverse currents=0°·03.
(Commonly=0).

The thermal capacity of the cell was determined and found to be equal to 68 grms. of water; so that

$$68 \times \cdot 466 = 31 \cdot 688 \text{ grms.}^\circ \text{C.}$$

represents the total mean heat evolved when the cell was worked forwards, and due to

$$\frac{c^2rt+ec}{J}.$$

The mean resistance of the cell was found to be 1·4 ohm, and taking e, the difference between E and E_1, as ·34 volt, we get

$$\frac{c^2rt}{J} = {\cdot}04 \times 1{\cdot}4 \times 1200'' \times {\cdot}24 = 16{\cdot}1 \text{ grms.}^\circ \text{ C.}$$

and

$$\frac{ect}{J} = {\cdot}34 \times {\cdot}2 \times 1200'' \times {\cdot}24 = 19{\cdot}58 \text{ grms.}^\circ \text{ C.}$$

Hence the sum of these amounts, 35·68 grms.° C., and their difference, −3·48 grms.° C., represent the total amounts of heat evolved with direct and with adverse currents respectively, with which the experimental results of 31·69, and practically 0, accord within the limits of experimental error.

Hence it results that a copper-silver nitrate cell gives an electromotive force below the calculated value, and when it sends a current a portion of the energy due to the replacement of silver by copper appears as heat in the cell, and its equivalent in E.M.F. is therefore wanting. Consequently, when the cell is worked backwards, a smaller amount of work than that represented by the negative heat of substituting silver for copper is capable of producing the required electrolysis, the difference being supplied at the expense of sensible heat.

In view of the great difference in the heat of formation of mercury salts found by me, and the previously recorded values, I determined the heat of replacement of silver by copper from a solution of $AgNO_3$, $200H_2O$. The amount of silver was accurately determined both by titration of the solution and by calculation from the weight of the dissolved copper.

The results of two exactly similar experiments gave

I. = 33,420 calories,
II. = 33,450 calories,

as the heat of replacement :—

$$Cu + Ag_2(NO_3)_2Aq = Cu(NO_3)_2Aq + Ag_2.$$

These numbers are slightly below those found by Thomsen, he giving 35,630 as a mean; but they are sufficiently in agreement to show that the actual E.M.F. furnished by a copper-silver nitrate cell is considerably less than the calculated value.

It has been above remarked that the "thermovoltaic constants" of silver vary greatly with the salt of the metal employed, the soluble salts having much larger constants than the insoluble, and the latter diminishing to an insignificant quantity in the case of the iodide, or of the chloride suspended in cupric chloride. This would therefore show that it probably does not depend in any way upon the metal itself acting as a thermoelectric junction, but upon the nature of the chemical changes occurring in the cell.

From these, and other considerations which are given later, I conclude that the thermovoltaic constant depends upon two prime causes. One, which operates with both soluble and insoluble salts of silver, is a tendency of that metal to form subsalts, *e.g.* subchloride Ag_2Cl, the heat of formation of the equivalent of which is greater than that of the normal salt, so that silver is more readily attacked superficially and acquires a lower relative potential than would be predicted from the heat of formation of its normal salt.

Wetzlar has shown that silver-leaf immersed in solutions of cupric or ferric chloride becomes converted into Ag_2Cl, so that when a silver plate is immersed in cupric-chloride solution its surface rapidly becomes covered with a film of the subchloride of sufficient thickness to prevent the metallic silver producing its usual effect, and thus the thermovoltaic constant practically vanishes.

The other cause of divergence is of much wider application, and appears to me to depend upon the fact that with any salt, such as silver nitrate, which does not form a hydrate with water, but being soluble absorbs heat on dissolving, this heat can be supplied by the thermometric heat of surrounding bodies, and one would expect it to be "bound energy," and to be a reversible thermal effect independent of the effects due to the "free energy" of the chemical change.

Since the heat of solution of silver nitrate is a large negative quantity, −5440 or −10,880 for $Ag_2(NO_3)_2,Aq$, the heat of formation of anhydrous $AgNO_3$ would be greater by that amount than that measured when using its solutions. This would give the heat of formation of

$$[Ag_2, O, N_2O_5] = 16{,}770 + 10{,}880 = 27{,}650,$$

and thus give a calculated E.M.F. about ·235 volt lower, still leaving a deficiency of about ·105 volt to be accounted for by the tendency of silver to form a film of subsalt; unless indeed the heat of formation of silver nitrate has been under-estimated, as the numbers given above for the heat of replacement of silver by copper might indicate.

These numbers, after allowance for the negative heat of solution of silver nitrate, would give a calculated electromotive force almost exactly in agreement with the experimental value.

Lead.

Lead, like silver, is a metal having a large negative "thermovoltaic constant," which also varies with the salt of the metal employed. Wright and Thompson give for the difference of E and E_h in the case of lead in saturated solutions of its chloride and bromide ·222 and ·235 volt, while for the iodide only ·002 volt. This difference, however, is readily explained on reference to the numbers selected by these authors in calculating the theoretical E.M.F., as, in the case of the chloride and bromide, they employ the heats of formation of the dissolved salts, while they use the heat of formation of solid lead iodide. Inasmuch as they used *saturated* solutions of the chloride and bromide, it is difficult to see why they selected the values corresponding to the dissolved salts, since any more salt formed must necessarily be produced in the solid state, and the electromotive force would, consequently, be determined by the heat of formation of the solid salts.

Further, since there is no chemical attraction between these compounds and water, and there is merely an absorption of heat on solution, in accordance with the view stated above (in regard to silver nitrate), the E.M.F. will depend upon the heat of formation of the solid salt, even though the solution be not saturated.

The values calculated from the dissolved and anhydrous salts and the experimental values are compared below :—

Observed
Average Value.
Volt.

$$\left.\begin{array}{ll}[Zn, Cl_2, Aq]-[Pb, Cl_2, Aq] & =36870\text{ c.}=\cdot 796\text{ volt}\\ [Zn, Cl_2, Aq]-[Pb, Cl_2] & =30070\text{ c.}=\cdot 649\text{ volt}\end{array}\right\}\cdot 591$$

$$\left.\begin{array}{ll}[Zn, Br_2, Aq]-[Pb, Br_2, Aq] & =36550\text{ c.}=\cdot 789\text{ volt}\\ [Zn, Br_2, Aq]-[Pb, Br_2] & =26510\text{ c.}=\cdot 572\text{ volt}\end{array}\right\}\cdot 571$$

The zinc-lead iodide cell being calculated from the heat of formation of solid PbI_2 accords almost exactly with the experimental value, the difference being only ·002 volt.

Similarly with a zinc-lead nitrate cell, while the E.M.F. calculated from the heat of dissolved lead nitrate is ·737 volt, after allowing for the large negative heat of solution, -7600, one gets for $[Zn,O,N_2O_5,Aq]-[Pb,O,N_2O_5]$, $102,510-(68,070+7600)=26,840$, giving a calculated E.M.F. of ·580 volt, which agrees exactly with the experimental value ·580 to ·591 volt.

A reference to the effect of concentration of the solutions on the electromotive force of a zinc-lead nitrate cell confirms my views. Wright and Thompson find that increasing the solution strength from ·25 to $2\,M''(NO_3)_2, 100\,H_2O$ causes an increase in the E.M.F. from ·580 to ·591, while they calculate that it should decrease from ·759 to ·716. Hence, instead of a *fall* of ·043 volt there is a *rise* of ·011 volt, the cause of the increase being in all probability due to the fact that the heat of dilution of zinc nitrate is positive in strong but negative in weak solutions; so that with moderately strong solutions the whole of the combining energy of zinc nitrate with water may be converted into electric energy, while in weak solutions a portion of that part of the energy of chemical change is expended in the absorption of heat due to the greater dilution of the salt.

It may be well to give here some collateral evidence in support of this view. In the first place, if lead in chloride solutions, either not saturated with or entirely free from *lead* chloride, had a tendency to form $PbCl_2$, represented by the heat of formation of the dissolved salt, viz. 75,970 c., this metal should be without action on a solution of stannous chloride, the heat of formation of which in aqueous solution is 81,140 calories; but, as a matter of fact, a piece of pure clean lead placed in an acid solution of stannous chloride becomes immediately covered with a dull grey film, and after a short time small brilliant crystals of deposited tin appear all over its surface. For the same reason a cell consisting of lead and tin plates, both immersed in either dilute hydrochloric acid, stannous chloride, or lead chloride solutions, furnishes a current the direction of which indicates that lead and not tin is the metal attacked, the actual E.M.F. being about ·008 to ·012 volt. On the other hand, pure tin will not remove lead from solutions of its nitrate or acetate or cold solutions of its chloride, though if an acid solution of lead chloride be boiled with tin, lead is deposited to a slight extent. Similarly the above lead-tin cell, using an acid solution of stannous chloride, will be found to diminish in E.M.F. as its temperature is raised, so that between 50° and 60° C. it possesses no E.M.F., while above that temperature the current flows in the reversed direction, tin being the metal attacked. This explains why an alloy of lead and tin, when treated with hot hydrochloric acid, yields a solution of stannous chloride, leaving the lead unattacked. That the non-production of lead chloride cannot be due to the prospective absorption of heat on dissolving *after its formation* appears sufficiently evident; and I think this reversed action at the higher temperature is to be explained by the heat of formation of the compound $SnCl_22H_2O$ being greater than that of even solid lead chloride (viz. 86,510 and 82,550 respectively); and although the former would be dissolved with a certain absorption of heat dependent on the strength of solution and the temperature, this amount would be less than the 5370 calories absorbed at low temperatures and in dilute solutions, and, further, being due to the merely physical process of solution, may at the high temperature be supplied largely at the expense of sensible heat, instead of being deducted from the energy of the chemical change.

Nickel is a metal in the formation of the anhydrous chloride of which 74,530 calories are evolved, while in aqueous solution it furnishes 93,700 calories. If, therefore, the heat of formation of the dry salt plus the heat of hydration be regarded as measuring the total chemical energy and as occurring together, nickel should be able to replace tin or lead from their chloride solutions. But, as a matter of fact, instead of this being the case, pure electro-deposited tin is capable of completely displacing nickel from a solution of its chloride containing free hydrochloric acid, when allowed to remain in contact for a considerable time; and on boiling the mixture of tin and deposited nickel in hydrochloric acid, tin and not nickel is dissolved. Nitric acid, however, rapidly attacks the nickel.

This result is at once explained on reference to the heat of formation of anhydrous nickel chloride, which, as above stated, is given by Thomsen as 74,530, while the heat of formation of stannous chloride is 81,140 in aqueous solution, or 80,790 if anhydrous. The heat which would on the whole be absorbed by this replacement in aqueous solution would doubtless be supplied by surrounding temperature.

These considerations, therefore, show that the heat of formation of the anhydrous salt is the most important factor in determining the chemical reaction, and suggest that the kind of energy set free in the formation of a dry salt differs from that due to the hydration of that salt, the latter being only partly "free energy," while the negative heat of simple solution may be supplied entirely by the "bound energy" of the chemical change.

A similar explanation will account for the fact that an iron-cadmium chloride cell gives an E.M.F. opposite in direction to that anticipated from the heats of formation of their salts in solution, since Thomsen gives $[Cd,Cl_2] = 93,240$, $[Cd, Cl_2, Aq] = 96,250$, and $[Fe, Cl_2] = 82,050$, $[Fe, Cl_2, Aq] = 99,950$.

Thus, taking the anhydrous salts we get a difference of 11,190 on the side of cadmium, while taking the dissolved salts gives 3700 calories in favour of iron being attacked and depositing cadmium, which is known not to occur. Thus the first action in point of time—the formation of the anhydrous salt—directs the course of the chemical change, which, however, in the present case is not a replacement of iron by cadmium, but if a current flow, hydrogen is evolved on the iron plate; so that this cell does not reverse the expected reaction and furnish a current at the expense of sensible heat, but, owing to the initial difference in the heats of formation of the dry salts, an entirely different reaction is started and continued.

It is owing to the fact that iron is not deposited electrolytically from neutral or acid solutions of its salts that its large positive constant was referred earlier in this paper to a false reaction, though the above consideration shows at least one cause for its anomalous behaviour.

Tin.

Measurements of the electromotive force of cells in which tin in a solution of its chloride is opposed to other metals having shown * that it possesses an apparent negative "thermovoltaic constant," it was resolved to employ a cell in which tin is the metal attacked, and which should consequently give an E.M.F. in excess of the heat evolution due to the chemical change. Copper, which naturally suggests itself as the opposing metal, is, however, very unsatisfactory for thermal measurements when used in a solution of its chloride, owing to the production of cuprous chloride on its surface, and further, as Thomsen found for the heat of replacement of tin by zinc from solution of its chloride numbers varying from 28,940 to 33,510 calories (a variation of 4570 calories, or ·098 volt), a determination of the previously unknown heat of formation of stannous sulphate was made with the object of using a tin-copper sulphate cell.

A solution of copper sulphate, each cubic centimetre of which contained ·015875 grm. of copper, corresponding to $\cdot 45 Cu\,SO_4\,100H_2O$, and containing free sulphuric acid to the extent of 2 per cent., was treated in the calorimeter with excess of pure granulated tin; the copper was speedily deposited, and the following heat measurements were made :—

Weight of Copper.	Heat evolved.	Heat of Reaction.
·635 grm.	190·36 grm.° C.	19,036 calories.
·635 "	193·14 "	19,314 "
·635 "	193·98 "	19,398 "
	Mean . .	19,250 "

As Thomsen finds for the heat of formation of copper sulphate 55,960, this number added to 19,250 gives the heat of formation of $[Sn, O, SO_3\,Aq] = 75{,}210$ c.

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* Phil. Mag. ser. 5, vol. xxi. p. 13.

In the above reaction there was no evolution of hydrogen gas, even after the whole of the copper had been deposited, and the conditions were as similar as possible to those obtaining in the case of the same replacement in the voltaic cell. But in the next experiment, which was conducted partly as a control on the previous values, a small amount of hydrogen was produced towards the end of the reaction. The solution of stannous sulphate obtained in the previous reaction was decomposed by placing in it strips of pure cadmium, and the heat of the reaction $\mathrm{Sn\,SO_4Aq + Cd = CdSO_4Aq + Sn}$ was accordingly measured, giving 15,210 c. as the result. Since $[\mathrm{Cd, O, SO_3\,Aq}] = 89{,}880$, the difference $89{,}880 - 15{,}210 = 74{,}670$ represents the heat of formation of $[\mathrm{Sn, O, SO_3\,Aq}]$, as measured in this way, and agrees fairly well with the result obtained by the replacement of copper by tin, but is slightly lower, probably owing to the above-mentioned slight evolution of hydrogen, and therefore excess of the heat of replacement of tin by cadmium.

As a further check upon the general method I determined the heat of replacement of copper by zinc, using the same solution, viz. $\cdot 45\mathrm{CuSO_4}\ 100\mathrm{H_2O}$, but, of course, free from sulphuric acid, and found the heat of reaction to be 50,980 c., the number calculated from Julius Thomsen's values being 50,130 for solutions containing $\cdot 25\mathrm{CuSO_4}\,100\mathrm{H_2O}$, which numbers I considered to be in sufficiently close agreement.

But adopting 19,250 c. as the heat of replacement of copper by tin we get a calculated E.M.F. of only ·416 volt, while experiment shows an E.M.F. of ·55 volt as a mean value; thus showing that a tin-copper sulphate cell furnishes an E.M.F. of about ·144 volt in excess of its computed value. Using this cell in a calorimeter, I found that with direct currents of ·1125 ampere passing through the cell there was an absorption of heat, and when worked backwards against the E.M.F. of the cell there was an evolution of heat apart from that due to the square of the current and the resistance. The internal resistance in one of these cells was reduced to only ·9 ohm, so that the heat due to it was very small.

I conclude from these observations, and in consonance with the theory already stated, that stannous sulphate has a negative heat of dilution, which portion of its total energy, while appearing as negative in the calorimeter, is unable to reduce the electromotive force due to the "free energy" of the undiluted salt, the negative heat of dilution being supplied by fall of temperature. I have not been able to verify this conclusion experimentally, and the subject is complicated by the necessity of having free acid present in its solutions to prevent the formation of basic salts. The effect both of dilution and of the quantity of free acid present also greatly influence the E.M.F. of stannous chloride cells, and some points in regard to its action may be seen from the following consideration.

It is well known that if a strip of tin be placed vertically in a vessel, containing at its lower part a strong solution of stannous chloride (with free hydrochloric acid) and above it water, at the line of junction of the liquids, where a dilute solution of stannous chloride is formed by the intermixture, metallic tin is deposited in brilliant crystals on the tin plate, tin dissolving in the liquid below. This solution of a metal in a concentrated solution of one of its salts and its deposition from a dilute one is entirely contrary to what would be expected, were it not for the fact that in the relatively strong hydrochloric acid in the lower liquid there is a store of potential chemical energy which may either evolve heat in diffusion and dilution, or, by attacking the tin plate in contact with it, furnish a current which, flowing through the liquid and metal, has the effect of depositing an equivalent amount of tin from the solution above, in which the hydrochloric acid, being more dilute, has less tendency to dissolve the tin, a considerable portion of its energy having already been expended in the heat evolved during dilution.

Hence in tin cells, while I recognize a negative "thermovoltaic constant" which is capable of producing reversible thermal effects in the cell, I am inclined to refer them to two causes: (α) a negative heat of dilution of the salts, and (β) to effects of differences of concentration of the acid necessarily employed to keep the salt in solution*.

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* In my previously published measurements of the E.M.F. of tin cells the value given for the cadmium-tin sulphate cell was only ·189 volt; but I have since found that, using more dilute and less acid solutions, a persistent value as high as ·234 volt may be obtained.

General Conclusions.

A review of the published measurements of the thermal values of chemical reactions, in particular those directly relating to electromotive forces, namely, the replacement of one metal by another from its dissolved salt, will, I think, show that however valuable they may be as guides they cannot be implicitly relied upon as furnishing absolute and unimpeachable data by which to judge the truth or fallacy of a given theoretical conclusion. In the first place, in the numbers representing the heat of formation of the same body, determined by different experimenters, there are frequently discrepancies of considerable magnitude. Thus Thomsen finds for the heat of reaction of lead acetate and zinc 34,950 cal., Favre finding only 31,200 c., while Andrews gave 37,710 c.; a total variation between the extremes of 6,510 calories. For the heat of formation of CuO, and therefore of all salts derived from it, Thomsen gives 37,160, Favre and Silbermann 43,770, and Andrews 38,300, while $[Fe, Cl_2, Aq]$ is variously estimated by Thomsen at 99,950, by Favre and Silbermann at 106,700, and by Andrews at 102,060.

Besides these experimental differences there is always the fact that, as Berthelot has clearly laid down in his introduction to his *Essai de mécanique chimique*, the quantity of heat evolved in a reaction measures *the sum of the physical and chemical changes* which occur in the reaction; some of which quantities may be positive or negative, and which may or may not directly influence the transformable energy of the chemical change.

Adopting the view of Helmholtz that a chemical process gives rise to a total amount of energy, part of which is freely transformable and part of which is "bound energy," while the calorimeter measures the total energy, the electromotive force of a voltaic cell is a measure of the actual transformation of free energy. In cases, therefore, in which a portion of the total energy is to be reckoned as negative in respect to the main reaction, we see that such portion may be supplied either at the expense of the free or of the bound energy. If performed by the free energy, obviously the amount of that energy which could appear in other forms must be proportionately reduced; while if performed at the expense of the bound energy, merely thermal effects would follow, without affecting the course or amount of the free-energy stream. It follows that in certain cases, as for instance in the case of lead nitrate, or silver nitrate, if the heat of solution is a large negative quantity, the free energy, in my opinion, is greater than the total energy, since the bound energy is negative in sign; but, assuming that this negative bound energy is not supplied at the expense of the free energy, it can be readily made up by thermometric heat. Hence, while thermal measurements indicate 27,600 − 10,880 = 16,720 as the heat of formation of silver nitrate in solution, the voltaic cell shows that at least 27,600 thermal units must be regarded as transformable free energy, the 10,880 negative heat-units being abstracted not from it, but from sensible heat. On the whole there must be an increase of entropy, but there may be an absorption of heat, as when a lead-copper nitrate cell sends a current, or a copper-silver cell is worked backwards.

The partial independence of thermal effects due to concentration of solutions and electromotive forces, is recognized by Helmholtz when he states in the case of a Latimer Clark cell with saturated zinc-sulphate solution, that "the zinc sulphate, newly formed by the current, can no longer be dissolved, and its latent heat of solution is saved; accordingly there is *a more powerful development of heat in the cell in spite of the weaker electromotive force.*" His theoretical mathematical treatment disposes of many difficulties, but it did not prevent him from the error of believing that mercury, when deposited by zinc, even from its solid salts, gave rise to an E.M.F. largely in excess of the thermal values, which, according to my view, would be quite impossible, as it would amount to a conversion not merely of "bound energy" into "free energy," but, under special circumstances of the thermometric heat, into higher forms of energy with the production of mechanical effect.

It might be thought that the whole of the heat liberated in the formation of the fully hydrated salt was a measure of the available energy, and that the negative heats of dilution were supplied solely at the expense of sensible heat, but this supposition is entirely negatived by the actual electromotive forces of several cells (*ex. gr.* zinc-cadmium bromide cell); so that I conclude that since the hydration and solution of a newly formed salt will, to a great extent, occur simultaneously, the heat of hydration is partly absorbed in the process of solution and dilution, and therefore to that extent cannot furnish electrical energy. On the other hand, when a salt is used which does not form hydrates, and consequently can *only* absorb heat during solution, the energy due to the formation of the solid salt determines the E.M.F., which cannot be subsequently decreased by a process which has no chemical continuity with it, but which merely absorbs sensible heat.

As Helmholtz has already remarked, the exact reversibility of these processes indicates the constancy of the partition of "free" and "bound," or electrogenic and thermogenic energy, and negatives the earlier supposition of Braun that the proportion of transformable energy was the accident of circumstances.

Helmholtz has further shown that, regarding the energy and entropy of a system undergoing a restricted change as represented by the differential coefficients of an integral function of the system or substance, under the usual conditions the performance of external work takes place at the expense of the "*free energy*," while loss of heat at the expense of the "*bound energy*." But whether there subsists a definite relation between the freely transformable and the bound energy on the one hand, and on the other hand between that portion of the energy set free by the process of hydration of a salt which is convertible into electric energy, and that portion which runs down into heat, is a question which appears to demand careful experimental as well as mathematical consideration; especially with reference to the influence of temperature and various degrees of concentration of the salt.

The fact that the E.M.F. of a cell, like zinc-cadmium chloride, does not increase with greater dilution as rapidly as it should from thermochemical considerations, may be taken as indicating that the degradation of energy of the system increases as the number of water-molecules in which the salt is dissolved is augmented.

I conclude, therefore, that:—

I. The primary factor in determining the electromotive force of a voltaic cell is the relative heat of formation of the anhydrous salts of the two metals employed.

II. That the E.M.F. may set up chemical changes of a different direction and character from those predicable from the heat of formation of the dissolved salts.

III. That the E.M.F. set up by (I.) may be, and usually is, supplemented by the energy, or a portion of the energy, due to the hydration or solution of the solid salts, and may have values which accord with the heat of formation of the dissolved salts.

IV. That in those cases in which there is no chemical attraction, or a very feeble attraction between the water and the salt, the negative heat of solution is derived from sensible heat, and is not supplied by the free energy of the chemical change. All cells in which such salts are employed opposed to zinc should have *negative* "thermovoltaic constants," and evolve heat when they send a current forwards.

V. That when metals, whose salts have purely negative heats of solution, are opposed to metals whose salts they can replace, the E.M.F. set up is *in excess* of the total thermal change. Such cells, therefore, absorb sensible heat when worked forwards.

VI. That, taking the foregoing facts into consideration, no cell exists which can furnish an E.M.F. in excess of the free energy of the chemical change; *i.e.* which can convert sensible heat into electric energy working at uniform temperature. (Negatives the supposition concerning mercury and other cells.)

VII. That certain metals have a tendency to form films of sub-salts on their surfaces, the formation of which giving rise, as it does, to a different thermochemical reaction, naturally furnishes an E.M.F. which does not correspond with the values calculated from the heats of formation of their normal salts. (*Ex. gr.* copper in cupric chloride, mercury in mercuric chloride, probably silver in most soluble chlorides.)

VIII. That the electromotive force of a voltaic cell furnishes a more accurate measurement of the "free energy," and therefore of true chemical affinity, than data derived from calorimetric observations.

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IV. That in those cases in which there is no chemical attraction, or a very feeble attraction between the water and the salt, the negative heat of solution is derived from sensible heat, and is not supplied by the free energy of the chemical change. All cells in which such salts are employed ought to have negative "thermovoltaic constants," and absorb heat when they send a current outwards.

V. That when metals, whose salts have purely negative heats of solution, are opposed to metals whose salts they can replace, the E.M.F. set up is in excess of the total thermal change. Such cells, therefore, absorb sensible heat when worked forwards.

VI. That, taking the foregoing facts into consideration, no cell exists which can furnish an E.M.F. in excess of the free energy of the chemical changes, or which can convert sensible heat into electric energy working at uniform temperature. (Negatives the supposition concerning mercury and silver cells.)

VII. That certain metals have a tendency to form films of sub-salts on their surfaces, the formation of which gives rise, as it were, to a different thermochemical reaction, resulting in an E.M.F. which does not correspond with the values calculated from the heats of formation of their normal salts. (E.g., copper in cupric chloride, mercury in mercuric chloride, probably silver in most soluble chlorides.)

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PROCEEDINGS

OF

THE PHYSICAL SOCIETY

OF LONDON.

APRIL 1889.

XIV. *On the Dimensions of Electromagnetic Units.*

By Prof. G. F. FITZGERALD, *F.R.S.*[*]

SOME attention has lately been called to the question of the dimensions of electromagnetic units, but the following obvious suggestion seems to have escaped notice.

The electrostatic system of units may be described as one in which electric inductive capacity is *assumed* to have zero dimensions and the electromagnetic system as one in which the magnetic inductive capacity is assumed to have zero dimensions. Now if we take a system in which the dimensions of both these quantities are the same, and of the dimensions of a slowness, *i. e.* the inverse of a velocity, $\left[\frac{\mathrm{T}}{\mathrm{L}}\right]$, the two systems become identical as regards dimensions, and differ only by a numerical coefficient just as centimetres and kilometres do. There seems a naturalness in this result that justifies the assumption that these inductive capacities are really of the nature of a slowness. It seems possible that they are related to the reciprocal of the square root of the mean energy of turbulence of the æther.

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[*] Read February 23, 1889.

XV. *Note on the Measurement of Resistance.*

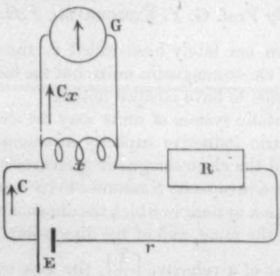
By Dr. J. W. W. Waghorn*.

It is well known that an unknown resistance x can be determined in terms of a known resistance R, if both are arranged in series with a cell, and if the deflexions of an electrometer be observed when connected in turn to x and to R.

If C is the current in the circuit, the differences of potential at the terminals of x and of R are respectively Cx and CR.

It is also well known that any voltmeter may be substituted instead of the electrometer, if the resistance of the voltmeter-circuit is very large compared to x and R.

But it may not have been generally observed that the same method may be employed whatever be the relative resistances of the voltmeter and the conductors; and is true even if the usual conditions are reversed, so that the voltmeter-resistance is very small compared to x and R. Any galvanometer may take the place of the voltmeter.



Let G be the resistance of the galvanometer;
E be the E.M.F. of the battery;
r be the resistance of the battery and connecting-wires.
Let C be the strength of the current on the undivided part of the circuit;
C_x be the strength of the current through the galvanometer when its terminals are connected to x;
C_R be the strength of the current through the galvanometer when its terminals are connected to R.

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* Read February 23, 1880.

Then

$$C = \frac{E}{R + r + \dfrac{Gx}{G+x}},$$

and

$$C_r = C \times \frac{x}{G+x} = \frac{E}{R + r + \dfrac{Gx}{G+x}} \times \frac{x}{G+x}$$

$$= \frac{Ex}{RG + Gx + rG + rx}.$$

Also, substituting R for x,

$$C_R = \frac{ER}{RG + Gx + rG + rR}.$$

If x and R are large compared to r, the denominators of the above fractions will be practically identical ; and the approximation will be still closer if, as would usually occur in practice, x is not very different from R.

The sensitiveness of the method is practically the same as that obtained in the ordinary "substitution" process of determining, but has the small advantage over the latter of not requiring a knowledge of the resistance of the galvanometer.

XVI. *On the Use of* Lissajous' *Figures to determine a Rate of Rotation, and of a Morse Receiver to Measure the Periodic Time of a Reed or Tuning-fork.*

By Prof. J. VIRIAMU JONES, *M.A.*[*]

IT is sometimes of importance to determine with great accuracy the angular velocity of a rotating body at a given instant. For instance, in measuring an electrical resistance in absolute measure by the British Association method or the method of Lorenz, the rate of rotation of the revolving coil or disk must be known with full accuracy at the time when the reading of a galvanometer-needle is taken.

The method I have to bring before the Physical Society in this note consists in obtaining, by means of Lissajous' figures, equality of period between the rotating body and a reed maintained in vibration electrically, and then subsequently determining the vibration-period of the reed.

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[*] Read March 23, 1889.

(i.) *The use of* Lissajous' *Figures to obtain Equality of Period between the Revolving Apparatus and the Reed.*

In one end of the axle of the revolving apparatus (which in the application of this method in the laboratory of the University College, Cardiff, is a Lorenz disk rotating about a horizontal axis) a pin is placed excentrically. The pin fits accurately in a hole in a rod free at one end, and constrained to move only longitudinally at the other.

Fig. 1 represents an end view of the axle M M M. The excentric pin is indicated by the dotted circle at B. The pin fits into a hole in the rod S S free at the end C, the other end, A, being constrained to move only longitudinally.

When the apparatus is in rotation the excentric pin impresses on the rod a vibratory motion; and the vibration-period of the rod is equal to the revolution-period of the revolving disk.

To the free end of the rod a mirror, T, is attached.

In the apparatus in my laboratory, already mentioned, the axle is horizontal and the rod moves in a vertical plane.

The reed is a steel bar 100 centim. long, and its section is rectangular, 1·51 centim. by ·60 centim. It is clamped in a massive iron frame, and by moving the bar in the frame the vibrating segment may be made longer or shorter so as to vary its vibration-period. The period may be further adjusted by the movement of a rider.

Fig. 2 is a plan of the reed and its electrical arrangements, which are a little different to those in ordinary use in order to allow, without inconvenient readjustment of the springs, the lengthening or shortening of the vibrating segment.

A A A is the steel bar.

C C the iron frame to which it is clamped by the bolts and nuts D, D. This stand is clamped to a large wooden block resting on a concrete floor.

F F F is a wooden piece attached at N N to the iron frame, and bearing the electromagnet M.

On the opposite side of the vibrating bar to the electromagnet there is a pair of springs S_1. One of the pair is always in contact with the bar as it vibrates, and the other, fitted with screw-adjustment, makes and breaks contact with the first. The first spring is connected through the electro-

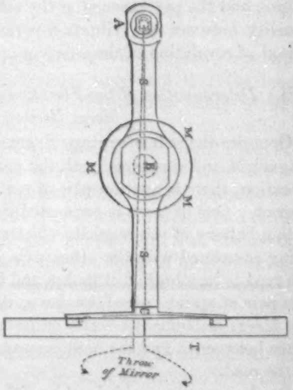

Fig. 1.—Vibrating Mirror on Disk. Full size.

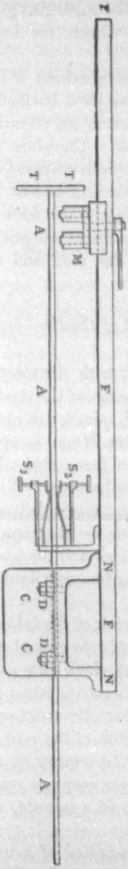

Fig. 2.—Reed and Stand. ¼ full size.

magnet with the pole of a suitable battery, the other pole being connected with the second spring. The apparatus gives no trouble, and once started the reed will vibrate for hours without attention.

To the free end of the vibrating segment a mirror T T is attached. In my apparatus the reed moves in a horizontal plane, and so its plane is at right angles to that of the vibrating rod attached to the axle of the rotating disk. Therefore the image of a spot of light seen by reflexion in both mirrors (viz. that attached to the rod on the axle, and that attached to the reed) will for synchronism be drawn out into some form of ellipse, and the permanence of the ellipse is an exact test of equality between the vibration-period of the reed and the period of revolution of the rotating-apparatus.

(ii.) *Determination of the Vibration-period of the Reed by a Morse Receiver.*

Opposite the pair of springs S_1 connected with the electromagnet M and concerned with the maintenance of the reed in vibration, there is another pair of springs S_2, similar in every respect. One of these is connected through a Morse receiver with a battery of the requisite electromotive force, the other being connected with the other pole of the battery. When the reed is in vibration it makes and breaks contact between this pair of springs; and we have, therefore, on the tape of the Morse instrument a series of dashes; a dash and the blank space between it and the next corresponding to one vibration of the reed.

On the same tape, side by side with the track of the inking-wheel of the Morse instrument, a pen is made to record the movement of the pendulum of the standard clock in a sufficiently ordinary manner. We have then merely to count the number of dashes in a given time, and we have the number of vibrations in that interval, and hence the pitch of the reed.

The electrical connexion is made in my laboratory by the escapement-wheel of the clock; and hence for accurate result the time taken must be an integral multiple of a minute, the period of revolution of the wheel.

This method of determining the vibration-period of a reed or tuning-fork has seemed to me to be of extreme simplicity, and by taking a sufficient interval of time may be made of any desired accuracy. A Morse receiver will spin out its tape at a speed of 40 feet a minute, and the ordinary rolls of tape will last for at least 15 minutes when it is doing so.

The limit of accuracy in this method of determining the rate of vibration is, I find at present, the constancy of vibration of the reed. It has been a surprise to me to find that the vibration-period is not constant to much more than 1 part in 1000.

In conclusion, I desire to express my thanks to Mr. John Gavey, of the S. Wales Telegraphic Department, for the help he has rendered me in connexion with this matter.

University College, Cardiff,
March 22nd, 1889.

Prof. Ayrton, whilst recognizing the extreme importance of determining speed accurately, suggested that the inconstancy of the reed may be due to the impulse being given at the end instead of at the middle of its swing; and recalled an experiment, shown before the Society by Prof. Perry and himself, in which the pitch of an electrically-driven fork was varied greatly by altering the adjustment of the contact-screw Referring to Dr. Thompson's modification where two tuning-forks drive each other, it was pointed out that the method requires the synchronism of the two forks to be very exact.

Mr. Blaikley inquired whether any doors were opened or closed during the experiments, as the pitch of a reed is affected by the size of its resonance-chamber. He also stated that the pitch of reeds driven pneumatically could be maintained constant to 1 part in 10,000, and mentioned that two forks nearly in unison influence each other's period when near together.

Referring to the two forks mentioned by Prof. Ayrton, Prof. S. P. Thompson said it was advantageous to mount such forks on sounding-boxes, for when placed at a suitable distance apart they then exert considerable mutual control. The sketch put on the board by Prof. Jones led Dr. Thompson to suppose that a perfect method of driving-forks had been devised; for two springs were shown touching opposite sides of the bar, and such an arrangement might be used to complete the circuit, only when the reed is in the middle position. He also believed that forks give greater constancy than single reeds, and mentioned some recent improvements in which one prong of an electrically-driven fork is made of phosphor-bronze.

Mr. T. H. Bakesley, reasoning from ideas suggested by Mr. Stroh's experiments on vibrating membranes, concluded that the periods of forks placed at $\frac{1}{4}$ wave-length apart would not influence each other.

In reply to a question from Mr. F. J. Smith, Prof. Jones thought there could be no "creeping" of the reed through the clamp. He also stated that he had been led to use a reed from the results obtained in Delaney's system of telegraphy, and the fact that Lord Rayleigh considered electrically-driven forks satisfactory.

XVII. *On Physico-Geometrical Models.*

By Prof. A. S. Herschel, *D.C.L.*, *F.R.S.*[*]

(Abstract.)

The subject had suggested itself to the Author as an important one for study some five years ago, from the possibility which he then discovered of effecting a four-limbed mechanical cycle, similar to a thermodynamic cycle, with a conically revolving pendulum, the mathematical conditions of whose motion involved the logarithm of the circle radius of the knob's revolution, as a quantity equivalent to entropy in the thermal cycle. The mechanical cycle may be effected as follows:—Suppose the pendulum-knob to be revolving at velocity V_1 and radius r_1; imagine the controlling force to decrease so that the radius varies from r_1 to r_2, the velocity remaining constant at V_1; then increase the controlling force to its original value, and simultaneously impart velocity to the knob to keep the radius constant at r_2, the final velocity being V_2. Next, further increase the controlling force so as to cause the radius to decrease to r_1 at velocity V_2, and, finally, decrease the controlling force to its initial value, simultaneously retarding the knob till the velocity is V_1 at radius r_1. The nett energy concerned in the operations was given as

$$W=(V_1^2-V_2^2)\phi,$$

where $\phi=\log\frac{r_1}{r_2}$; and the cycle affords an illustration of transformation of gyratory motion into energy of reverberatory push along the axis. The logarithm ϕ is also measured by the area of a hyperbolic sector, and is connected (as was shown) with a sector of the circle which a lamp's conical beam would inscribe upon a square glass screen or plate, dividing a long room of square cross-section into two parts. The conical beam prolonged through the glass then traces on the walls beyond hyperbolas, whose sectors from their summits are related to the circle's corresponding sectors by a well-known hyperbolocyclic trigonometrical connexion. From this mode of constructing the connexion, and from a discordance which it shows at the asymptote extremities with Euclid's definition of parallel straight lines, the Author concludes that cubic space cannot be continuous in its structure, but must be, in a physically constructive sense, like material particles, indestructively atomic or molecular. The models showed modes of constructing cubic space in various ways, chiefly by means of tetrahedra, octahedra, and dodecahedra ; the common element of form in these being also shown to be the right-angled tetrahedron or "biquoin," obtained by dividing a cube into six equal parts by three planes through its diagonals. A model of Sir W. Thomson's soap-form figure, and some wooden models representing Haüy's polyhedral atoms and their combinations, were used to illustrate the structural view thus taken of geometry ; and chemical-figure models of the ring of tetrahedral carbon molecules in benzene, and of the asymmetric groups of similar atoms in active and inactive tartaric acid were shown.

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[*] Read February 9, 1889.

XVIII. *The Rotation of the Plane of Polarization of Light by the Discharge of a Leyden Jar.* *By* DR. OLIVER LODGE*.

THE current produced by the discharge of a Leyden jar is so violent while it lasts, that those phenomena which depend upon the value of a current independently of its duration are well excited by it. Such are the induction of currents, the production of magnetism, and the rotation of the plane of polarization.

Nothing is easier than to wind a quantity of thin gutta-percha-covered wire round a piece of heavy glass, and to witness the bright flashing of a dark field between polarizer and analyser whenever a large Leyden jar is sparked through the coil, the source of light being a paraffin-lamp or gas-flame. The suddenness of the effect suggests, of course erroneously, that it is an illumination caused by the light of the spark which one is looking at.

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* Read March 9, 1880.

The fact that the discharge is oscillatory, and that the restoration of light in the dark field is oscillatory too, is proved by the fact that an adjustment of the analyser to one side or the other of complete darkness has just the same effect on the result. It is proved also by the fact that a biquartz exhibits no change in its sensitive tint during the discharge; similarly also Jellett, and other such reversible detectors, would be useless for the purpose: the effect is an irreversible one. Nevertheless some sort of measure of the effect can be made by finding the position of the analyser whereat the brightness of the field suffers no appreciable change at the occurrence of the spark, because in one position the oscillation on one side will darken it just as much as that on the other side brightens it.

It seemed to me possible that if the effect could be rendered pretty considerable a slight *darkening* of the field might be obtained with some adjustments, because some arcs of the sine curve have an average ordinate less than their middle ordinate. But when a fairly bright field winks slightly it is not easy to say whether it winks brighter or darker, and after all I do not know that it much matters.

The main interest in the experiment seemed to me to lie in the evidence it afforded of practical instantaneity in the development of the property in the substance under examination; and in order to try oscillations of much greater frequency than those I first used, I got my assistant, Mr. Robinson, to make a long tube of carbon disulphide with which to repeat the experiment in a sensitive manner.

Now, a most interesting experiment of Villari[*], in which he whirled a drum of heavy glass up to 200 revolutions per second between the poles of a magnet, and perceived the electro-optic effect to diminish from 100 revolutions per second upwards, and ultimately nearly cease at high speeds (say 180 per second), had led one to suppose that some distinct time was necessary for the production of the effect—something between $\frac{1}{800}$ and $\frac{1}{400}$ second. But on referring to that most useful summary of Electrical Science, Prof. Chrystal's article in the Encycl. Brit., I found, along with a quotation of Villari's experiment, a statement that Professors Bichat and Blondlot *, of Nancy, had by means of Leyden-jar discharge proved that practically no time was necessary. I accordingly procured a copy of the volume of the *Comptes Rendus* from London, and there found an all too brief account of a most beautiful series of experiments, by which they considered it proved that if any time is required, it is less than the $\frac{1}{30,000}$ second. The skill of these French Professors in optics, and their previous researches in connexion with the Faraday effect in various substances, are well known; and what I have to show to-day is practically nothing more than a repetition of their experiments with the theory worked out.

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[*] Pogg. *Ann.* cxlix. 1873, p. 324. Translated from the *Rendiconti Istit. Lombardo*, ser. 2, vol. iii.

I take either a bit of heavy glass in its helix, or, for projection, a tube of CS_2 a yard long surrounded by four large helices, each containing about 80 yards of gutta-percha-covered No. 16 wire; and on passing the discharge from a battery of several jars the field flashes out bright, in what may be (if one is looking direct towards the hot lime) quite a dazzling manner.

I have just received a post-card from M. Bichat, in answer to an inquiry, saying that the coil they used was the secondary bobbin of a Rühmkorff coil with a resistance of 5000 ohms. Now, whether this was by accident or by design, it was difficult for them to use a coil more suited to the purpose, or one that would give a larger effect, as I shall show directly. The Leyden jars employed by them were either one or two, about 18 inches high and 6 inches in diameter. We shall find that the effect increases in direct proportion to the capacity of the jars.

To find out whether any time was required for the development of the effect, they made the light coming through the tube illuminate a slit, the spark being made to illuminate another slit close above the first, and then both slits were examined in a rotating mirror. Both were spread out into a discontinuous band, and the serrations of the one agreed as nearly as could be seen with the serrations of the other. Thus proving in a beautiful manner that there was practically no lag of the effect behind its cause, and thereby contradicting the conclusion of Villari *.

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* *Comptes Rendus*, xciv. 1882, p. 1590.

Meanwhile I had been doing similar experiments, but with a bobbin of much smaller inductance, and using a still smaller capacity, my object being to find the greatest frequency able to show the effect distinctly. If, for instance, heavy glass or CS_2 was able to follow oscillations of some million per second, there could be no further question but that Villari's conclusion was wrong. I find that the CS_2 is able to show the effect when the rate of alternation is 70,000 per second ; and though the short length of heavy glass available does not enable me at present to make quite the same statement for it, I have no reason to suppose it in any way inferior. In fact, experiment distinctly suggests that the effect is practically instantaneous ; but as to the degree of instantaneity I shall be able to make a more exact numerical statement later on.

It may be of interest to the Physical Society to have the oscillatory character of the restored light demonstrated, and there is no difficulty in the experiment. One sets the analyser to as near darkness as possible, one receives the trace of residual light upon a rotating mirror, by which it is spread out into a faint band ; and on then sending sparks through the coil of wire round the tube of CS_2, the band brightens and presents a distinctly beaded appearance at every spark.

Rotating the analyser a little, every alternate bead grows fainter, while the other alternate ones brighten, thus proving most directly the oscillatory character of the light and of the Leyden-jar discharge.

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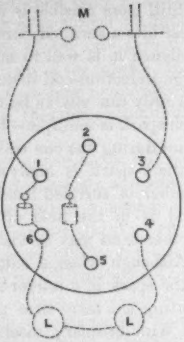
* They also mention an unpublished experiment by MM. Curie and Ledeboer, in which a disk of glass was spun between the poles after the manner of a Foucault disk of copper, so that the path of the light was parallel to the axis of rotation, instead of perpendicular to it as in Villari's drum arrangement, and in that case no diminution was observed. If this experiment is yet published I am ignorant of it.

Still more feasible is it to spread out the light of the spark itself into a serrated band, but in making this visible to an audience it is well to save time by exciting the jars with a large induction-coil instead of a Wimshurst machine, because not only can sparks be thus got much more frequently, but each spark is multiple—the jars filling and overflowing several times during the one coil-discharge. The multiple or intermittent spark is analysed by the revolving mirror into a number of serrated bands one after the other, and while one band is in the field of view of one part of the audience another band may be visible to another.

Although when a suitable circuit is employed the analysis of the spark in a mirror such as is used for manometric flames rotating not more than three or four times a second is easy, yet with ordinary discharging circuits I have used small mirrors spinning 200 times a second and failed to see any certain trace of oscillation; while, as is well known, Wheatstone used mirrors rotating 800 times a second, and got the image-spark only barely elongated: not in the least serrated. It may be worth while, therefore, to state the kind of circuit which I have recently employed. The capacity consists of a couple of condensers built up in the laboratory with double thicknesses of window-glass alternating with tinfoil, and the whole flooded in paraffin till it is a solid mass in a box of teak weighing a couple of hundredweight. These condensers stand a considerable length of spark, 3 or 4 inches for instance, but their strength is not in the least called out in the experiments now related. Their capacity when joined up parallel is ·048 microfarad. These are often supplemented by a battery of ordinary Leyden jars and by single jars, which altogether raise the capacity to ·074 microfarad or 670 metres. To vary the capacity in the ratio 1, 2, 4, I use a switch consisting of 6 glass pillars, each with a bit of close-fitting glass tube cemented on the top to serve as a mercury cup. The leading wires are held in the mercury cups by indiarubber umbrella-rings round the glass pillars; and insulated wire bridges easily make connexion between one pillar and the next.

In the annexed figure dotted lines indicate sufficiently the



permanent connexions. M being machine and discharger, L being circuit and coils. The movable connexions are made in pairs as follows :—

For series or cascade, connect 2, 3; 4, 5.

For one condenser only, connect 3, 4.

For both condensers in parallel connect 1, 2 ; 3, 4 ; 5, 6.

As regards circuit, one of the coils I use is a hank of 440 yards of thickly covered No. 16 G. P. wire as it came from the maker, with a self-induction of ·048 secohm and a resistance of 3·75 ohms.

The coils on the CS_2 tube consist of the same kind of wire ; they have a resistance rather greater than the above, and a combined self-induction of about ·008 secohm.

A number of gigantic electric-light cables have been inserted in the circuit at different times, one on a bobbin like two cart-wheels being kindly lent by the Engineers of the Liverpool Electric Supply Company ; but the easiest way of getting very large self-induction is to use No. 20 or No. 22 G. P. or I. R. wire not too thickly covered. I have recently obtained a large coil wound so as to give maximum self-induction which I estimate as 7 or 8 secohms ; I have not yet used this, but I frequently use a number of coils of No. 21 wire packed together as close as is easily possible, and making a total self-induction of 1 secohm or ten thousand kilometres.

Without going into further details, I may say that the observed frequency of the oscillations, as estimated from the appearance of the serrations in the revolving mirror, or by the pitch of the musical note accompanying the spark, agrees very respectably with the frequency as calculated from

$$\frac{3\times 10^{10}}{2\pi\sqrt{\left(\frac{L}{\mu}\cdot\frac{S}{K}\right)}}.$$

Returning now to the magneto-optic effect of the Leyden-jar discharge, I make out its theory to be as follows :—

The current at any instant during the discharge is

$$C=\frac{V_0}{pL}e^{-mt}\sin pt \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (1)$$

$$\text{where } m=\frac{R}{2L} \text{ and where } m^2+p^2=\frac{1}{SL}.$$

The difference of magnetic potential between the two ends of a rod of length l surrounded by a very long solenoid of n_1 turns of wire on unit length of it is

$$4\pi\mu Cn_1l.$$

The Verdet constant for carbon disulphide, or the rotation of the plane of polarization per unit difference of magnetic potential is, according to the determination of Lord Rayleigh, ·04202 minute for a temperature of 18° Centigrade, and for sodium light. Calling this k, we have the rotation effected by the current

$$\theta=4\pi kn_1l\mu C. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

In the case of the heavy glass experiment a helix longer than the stalk of glass is employed ; but in the case of the long tube of CS_2 and four helices there will be some correction necessary for the ends of the helices. This correction is, however, well understood, and there is no special need to introduce it at present.

Now the total amplitude of the light falling upon the analyser being a (this amplitude being all stopped if the analyser is set to darkness, and all transmitted if it be turned 90°), the component amplitude which will at any instant get through the analyser set to darkness, when the plane of polarization has been rotated through an angle θ, is

$$a \sin \theta \,;$$

and the amount of light which gets through during the time τ during which an impression is capable of being accumulated on the retina, *i.e.* before it has begun to die away as fast as it is produced, is

$$\int_0^\tau (a\sin\theta)^2 dt.$$

The amount of light which would get through in the same time if the analyser were set to maximum brightness would be

$$a^2\tau.$$

These expressions, then, furnish the measure of the respective effects upon the retina; and so we have

$$\frac{\text{apparent brightness restored by the spark}}{\text{maximum brightness possible}}=\frac{\int_0^\tau a^2\sin^2\theta\,dt}{a^2\tau}.$$

Now inasmuch as τ is comparable to the time of persistence of retinal impression, being perhaps equal to it, and since this time is greatly longer than any ordinary duration of a spark-discharge, the above ratio of the relative brightnesses reduces, for practical purposes, to

$$\mathrm{B}=\text{relative brightness}=\frac{1}{\tau}\int_0^\infty \sin^2\theta\,dt, \quad . \quad . \quad . \quad (3)$$

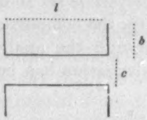
and this is what the eye will observe.

Referring back to equations (1) and (2) we have the means of determining this quantity. I do not know how to do it completely, but for the case when θ is moderately small the integral is easy, viz.:—

$$\mathrm{B}=16\pi^2k^2\mu^2n^2\frac{\mathrm{V}_0^2}{p^2\mathrm{L}^2\tau}\int_0^\infty e^{-2mt}\sin^2pt\,dt$$

$$=160\,k^2\mu^2n^2\,\frac{\frac{1}{2}\mathrm{S}\mathrm{V}_0^2}{\mathrm{R}\tau}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

The effect thus depends directly on the square of the total number of turns of wire employed, directly on the energy of the static charge used, and inversely on the resistance of the circuit.



To find the best size of wire to wind on a bobbin of given size, for the purpose, one can write down the value of n^2/R; given the length of the bobbin as l, its depth of winding-space b, the diameter of its empty core c. Call the radius of the uncoveredwire used ρ, and its radius when covered ρ'.

First, supposing no appreciable resistance in the rest of the circuit, it comes out

$$\frac{n^2}{\mathrm{R}}=\frac{lb}{4(c+b)}\left(\frac{\rho}{\rho'}\right)^2, \quad . \quad . \quad . \quad . \quad . \quad (5)$$

which means that the size of the wire does not matter, but that it is important to keep the covering thin. Only then unfortunately the discharge is liable to burst the covering. In my helices $\rho'=3\rho$, which is rather excessive, because it reduces the visible effect to one ninth of what it might be if no covering were used.

It is impossible to press this to its natural consequence of using only one turn of thick sheet copper, because the resistance of the rest of the circuit has been neglected. Taking it into account, as r, the relation becomes

$$\frac{n^2}{\mathrm{R}}=\frac{\dfrac{lb}{4(c+b)}\cdot\left(\dfrac{\rho}{\rho'}\right)^2}{1+\dfrac{4r}{lb(c+b)}(\rho\rho')^2}, \quad . \quad . \quad . \quad . \qquad (6)$$

which contains the product of the sizes of covered and uncovered wire as well as the ratio; and this product occurs in the term containing r, the external resistance. Hence, to keep this term small, it is desirable to use wire thin enough to throw the major part of the resistance into the bobbin; and there is no limit to the thinness of the wire that may be advantageously employed, until the thickness of the covering bears too high a ratio to the whole. And inasmuch as insulation thickness may be more judiciously distributed between layers than between consecutive turns, it is obvious how extremely suitable a coil for the purpose is the secondary of a Rühmkorff.

I said that I did not know how to evaluate the complete integral involved in (3) when θ is not small; but, as usual, I sent the problem to my brother, and he speedily reduced it to a form equivalent to this:—

$$\mathrm{B}=\frac{\mathrm{L}}{\mathrm{R}\tau}\int_0^{\mathrm{A}}\frac{1-\mathrm{J}_0(x)}{x}\,.\,dx, \quad . \quad . \quad . \quad . \qquad (7)$$

where

$$\mathrm{A}=8\pi kn\mu\mathrm{V}_0\sqrt{\left(\frac{\mathrm{S}}{\mathrm{L}}\right)}.$$

I have asked him to write a short appendix to this paper.

Returning now to consider the meaning of these equations, and attempting a numerical estimate of what to expect in practice, we shall find that though the instantaneous rotation expressed by (2) is enormous, being quite possibly 60 or even 180 degrees, yet the restoration of light expressed by (4) is but feeble, and only some ten thousandth part or so of what could be gained by rotating the Nicol. This explains why it is fairly easy to analyse the restored light into a beaded band, because then one gets the effect of the separate oscillations; and they are very bright though very momentary.

Looking at the formula (4), it is clear that to get large effects it is desirable to use a large battery of jars, and to charge to a high potential, only that one is afraid of breaking down the coil. Large capacity and a great number of turns of wire are the safest ways of increasing the effects.

One sees also that extra self-induction in the circuit does neither good nor harm to the resultant effect. It diminishes the effect of each oscillation, but it prolongs the time during which they last. And it is the total "*action*" of the series of decaying swings which the eye perceives. One sees that extra resistance in the circuit is wholly bad.

As to the cause of what must now be regarded provisionally as the erroneous conclusion of Villari I see no reason to doubt his experiments, though he does not give sufficient details to enable one to arrive at a perfectly satisfactory judgment on all points; it is practically certain that he did get a much diminished effect on spinning the flint-glass drum between the poles of the magnet and sending the light along successive diameters of the drum.

But the cause of this I venture to suggest is possibly to be found in the state of strain into which the glass will be thrown by centrifugal force. It may be said that, if so, the light ought to have been similarly affected even when no magnetic field was employed; and Villari expressly says that this was not the case.

But then it is to be noticed that, when no magnetic rotation is attempted, the aspect of the plane of polarization to the stress remains constant throughout the journey; and if light happens to enter with no component modifiable by the stress, it will go out in the same condition. Whereas when rotation has taken place inside the glass this constancy of aspect is destroyed, and the light on exit has a different component modifiable by the strain to what it had on entrance.

I do not profess to be able to give a coherent account of how this cause shall give rise to a reduction in the rotation instead of to an elliptical polarization. But then neither am I able to extract from Villari's account of his experiments any assurance that some elliptic polarization was not produced, and that the reduction of the rotation of the polarized plane was anything more than a mixture of small effects not easily analysable nor precisely defined.

It is in any case a most interesting experiment, and should be repeated so as to really get at the bottom of the cause of the observed phenomenon. There are many other experiments on whirling glass which may likewise be made.

APPENDIX, *by* ALFRED LODGE, M.A., *Coopers Hill, Staines.*

The value of

$$\int_0^\infty \sin^2\theta\, dt, \text{ where } \theta = Ae^{-mt}\sin nt, \text{ and } C = \frac{An}{\sqrt{m^2+n^2}},$$

is
$$\frac{1}{2m}\left(\tfrac{1}{2}.C^2 - \tfrac{1}{4}.\frac{C^4}{(\underline{|2})^2} + \tfrac{1}{6}.\frac{C^6}{(\underline{|3})^2} - \tfrac{1}{8}.\frac{C^8}{(\underline{|4})^2} + \dots\right).$$

For

$$\int_0^\infty \sin^2\theta\, dt = \tfrac{1}{2}\int_0^\infty (1-\cos 2\theta)\, dt$$

$$= \tfrac{1}{2}\int_0^\infty \left(\frac{(2\theta)^2}{\underline{|2}} - \frac{(2\theta)^4}{\underline{|4}} + \dots\right)dt.$$

The general term of this series is

$$\frac{2^{2p}A^{2p}}{\underline{|2p}}\int_0^\infty e^{-2mpt}\sin^{2p} nt\, dt,$$

and can be integrated by successive reduction.

Let u_{2p} denote

$$\int_0^\infty e^{-2mpt}\sin^{2p} nt\, dt\,;$$

then, integrating by parts,

$$u_{2p} = \frac{-e^{-2mpt}}{2mp}.\sin^{2p} nt\Big]_0^\infty + \int_0^\infty \frac{e^{-2mpt}}{2mp}.2np\sin^{2p-1} nt.\cos nt\, dt$$

$$= 0 + \frac{n}{m}\int_0^\infty e^{-2mpt}\sin^{2p-1} nt.\cos nt\, dt\,;$$

and again integrating by parts,

$$=-\frac{n}{m}\frac{e^{-2mpt}}{2mp}\,.\,\sin^{2p-1}nt\,.\,\cos nt\Big]_0^{\infty}$$

$$+\frac{n}{m}\int_0^{\infty}\frac{e^{-2mpt}}{2mp}\Big((2p-1)\sin^{2p-2}nt\cos^2 nt-\sin^{2p}nt\Big)\,.\,n\,.\,dt$$

$$=0+\frac{n^2}{2m^2p}\int_0^{\infty}e^{-2mpt}\Big((2p-1)\sin^{2p-2}nt-2p\,.\,\sin^{2p}nt\Big)\,dt$$

$$=\frac{n^2}{m^2}\cdot\frac{2p-1}{2p}\,.\,u_{2p-2}-\frac{n^2}{m^2}\,.\,u_{2p}.$$

$$\therefore\quad\left(1+\frac{n^2}{m^2}\right)u_{2p}=\frac{n^2}{m^2}\cdot\frac{2p-1}{2p}\,u_{2p-2}\,;$$

$$\therefore\quad u_{2p}=\frac{n^2}{m^2+n^2}\cdot\frac{2p-1}{2p}\,.\,u_{2p-2}$$

$$=\left[\frac{n^2}{m^2+n^2}\right]^p\cdot\frac{2p-1}{2p}\cdot\frac{2p-3}{2p-2}\cdots\tfrac{3}{4}\cdot\tfrac{1}{2}\int_0^{\infty}e^{-2mpt}\,dt$$

$$=\frac{1}{2mp}\left[\frac{n^2}{m^2+n^2}\right]^p\cdot\frac{(2p-1)(2p-3)\ldots 3\,.\,1}{2p(2p-2)\;\ldots\;4\,.\,2}=\frac{\underline{|2p}}{2^{2p}\underline{|p}\,\underline{|p}}\,;$$

$$\therefore\quad\frac{(2\mathrm{A})^{2p}}{\underline{|2p}}\,u_{2p}=\frac{\mathrm{A}^{2p}}{2mp(\underline{|p})^2}\cdot\left[\frac{n^2}{m^2+n^2}\right]^p=\frac{\mathrm{C}^{2p}}{2mp(\underline{|p})^2}\,;$$

$$\therefore\quad\int_0^{\infty}\sin^2\theta\,dt=\frac{1}{2m}\left(\tfrac{1}{2}\,.\,\mathrm{C}^2-\tfrac{1}{4}\,.\,\frac{\mathrm{C}^4}{(\underline{|2})^2}+\tfrac{1}{6}\,.\,\frac{\mathrm{C}^6}{(\underline{|3})^2}-\ldots\right).$$

Q. E. D.

N.B.—The symbols used for the constants throughout this Appendix have no connexion with those in the main paper, except that *m* is the same in both. The value of the C of the Appendix, expressed in terms of the symbols used in the paper, is

$$4\pi kn\mu \mathrm{V}_0\sqrt{\mathrm{S}}\div \mathrm{L}.$$

Mr. Ward mentioned that experiments similar to Villari's were now being carried out at the Cavendish Laboratory, a disk of glass being rotated about 200 times per second by means of a turbine. The results so far obtained do not confirm

Villari's, but owing to difficulties in keeping the speed constant it is impossible to make exact measurements. It has, however, been found that the strain due to centrifugal force rotates the plane of polarization and elliptically polarizes the beam, and that passing an alternate current round a stationary glass bar produces a distinct rotation of polarized light passing through it.

Referring to the oscillatory discharge of a jar, Prof. Rücker directed attention to Dr. E. Cook's experiments, described before the Society in June 1888, when photographs showing the dust-figures produced by sparks were exhibited, and pointed out that the frequency required to produce air-waves of the length there indicated was of the same order as the rate of oscillatory discharge, viz. about 1 million per second. Prof. Rücker also wished to know whether glass behaved precisely like CS_2.

Dr. Lodge said his experiments were not exact enough to decide the latter question, and mentioned that Mr. Chattock had some time ago produced dust-figures in tubes by jar-discharges, and shown that the wave-length depended on the capacity and self-induction.

Prof. Ayrton suggested the use of a phonograph as a means of recording and reproducing the oscillations, in the same way as himself and Prof. Perry have analysed the current curves of alternating dynamos. The discharge could be passed through a small coil fixed to a diaphragm and placed near a coil through which a steady current was passing, the attractions and repulsions serrating the surface of the rapidly revolving cylinder. By means of a mirror attached to a delicate magnifying-spring, the section of the surface may be determined.

Dr. Thompson remarked that it was satisfactory to learn that Villari's results admit of an interpretation other than by time-effect, and thought it advisable to vary the experiment by rotating a bar of glass ; but Mr. Ward said he attempted that experiment four years ago, and abandoned it on account of the enormous speed required.

XIX. *On an Electrostatic Field produced by varying Magnetic Induction.* *By* Dr. Oliver Lodge[*].

In common no doubt with many others, I have long wished to find some connexion between static electricity and magnetism. My early notions in this direction were such ideas as the following:—

(1) To spin a long bar-magnet on its axis, suspending in its field a sort of quadrant-electrometer needle charged oppositely at either end, and to look for a deflexion. The iron core of an electromagnet was supposed to serve.

(2) To construct a cylindrical or drum-shaped horseshoe-magnet like two iron wheels joined at the nave, and wound with wire there; to spin this on its axis and suspend a charged pith-ball between the two rims[†].

(3) To use a varying magnet instead of a moving one [quite probably this was suggested by Prof. Fitzgerald in a conversation I had with him some ten years back]; for instance, to suspend a charged gold leaf between the edges of the pole-pieces of an electromagnet, and watch it through a microscope when the exciting current is started or stopped.

Many other varieties of the experiment are in my note-books, but these are enough to show the idea.

The importance of the experiment seemed to me (rightly or wrongly) to lie in a possible crucial criterion between Maxwell's theory and some of the contemporary German theories; because it seemed to deal with something less than an open circuit, viz. no circuit at all.

It also struck me as a sort of converse experiment to that which Prof. Rowland has recently and so successfully carried out: the classical experiment of deflecting a magnetic needle by a rotating charged disk.

This can hardly be considered a connexion between electrostatics and magnetism, because a static charge in motion, if not a contradiction in terms, is essentially a current; and what the experiment proves is the truth of Maxwell's view that all electricity in motion, no matter whether by conduction, displacement, or convection, is a true current, and exerts magnetic effects. I should rather say that Rowland's experiment proves the truth of this view of Maxwell so far as convection is concerned. The question of displacement is a ticklish one just now, upon which I shall be safer to be silent*.

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[*] Read May 11, 1889.

[†] I find, on reference to Dr. S. P. Thompson's book, that one may describe this arrangement as the field-magnet of a Mordey dynamo.

But, now, think of a charged gold leaf hanging between the poles of a magnet. If the gold leaf move it constitutes a current, and therefore at once feels a force urging the current (*i. e.* the line of motion) across the lines of force in the orthodox way. Instead of supposing the gold leaf to move, let the magnet move. The same thing will happen, because the relative motion is the same as before. (If absolute motion of the static charge through æther is essential, then this last statement falls flat and may possibly be false.)

If this force were observed, it would be a connexion between magnetism and a static charge, provided any motion of the charged body due to the earth's motion in its orbit were shown to be inoperative or unessential.

Or, instead of moving the magnet, let its strength be varied; lines of force pass by the charged body, unless it is symmetrically situated, in this case as in the other; and hence the effect may naturally be expected to be the same in either case.

The experiment I wished to try was therefore not exactly a converse of the Rowland experiment, but was closely related to it.

Two years ago my friend Mr. A. P. Chattock came to Liverpool to act as Demonstrator, and I found him imbued with similar notions, and also with more practical ideas for carrying them out. Especially was he convinced of the fact that an actual magnetic field in which to suspend the charged body was entirely superfluous, but that varying magnetic induction in a region near the charged body would be equally effective and easier to manage. He wished to use, in fact, a closed magnetic circuit with all its lines enwrapped in itself, and to hang the charged body near but outside the circuit, so that no lines of magnetic force really ever passed through the charge, at least when the magnetic induction was steady. But when the magnetic induction was varied, say by reversing a magnetizing current round the closed magnetic circuit or ring solenoid, Mr. Chattock pictured lines flashing past the charged body in enormous numbers and producing the desired effect.

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* I say this with reference to what went on at Bath, not with reference to anything that has happened since. This paper was written two or three months ago, before I had heard anything of a communication from Dr. S. P. Thompson to the Royal Society.

I am bound to say that the idea of the closed magnetic circuit was not natural to me at first, and I resisted the views of the inventor of the magnetic potentiometer for the best part of a week. At the end of that time my conversion had taken place; and from that time to this some sort of closed magnetic circuit, with one or other form of charged body inside it, has been set up in my laboratory and experiments made with it from time to time.

The theory of the effect I have observed it may be useful just to write down in the most elementary manner.

The magnetic induction in a solenoid being I, the E.M.F. induced in any complete circuit round it, when I changes, is

$$e=\frac{d\mathrm{I}}{dt}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

If an E.M.F. act on a statically charged body at a distance r from centre of solenoid, the work done in driving it once round a circle is

$$e\mathrm{Q}=2\pi r\mathrm{F}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where F is the mechanical force exerted on the charged body.

Now if the E.M.F. in (2) is the same as in (1), and is caused by the induction I being either generated or destroyed, it will only last a very short time, and we must therefore consider what is the whole impulse of the force during the time it lasts, viz.

$$\phi=\int_0^\infty \mathrm{F}dt=\frac{\mathrm{IQ}}{2\pi r}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

And this is the momentum generated whenever the solenoidal magnetism is generated or destroyed. This therefore, or some simple multiple of it, is what is observed in the experiment.

Now in calculating out a numerical value for this we shall write

$$I = \frac{4\pi n C A \mu}{l} = \mu C \text{ times a length,}$$

and
$$Q = SV \qquad = KV \text{ times a length.}$$

Hence in the product IQ occurs the product μK, and this is the reciprocal of the square of the velocity of light.

This amply accounts for the smallness of the observed effect. By various devices the geometrical parts of I and Q can be increased somewhat to make the observation more easy, but nothing can get over the fact that the velocity of light squared occurs in the denominator of the expression.

To return to the account of experiments devoted to finding this effect. If there is any importance in the result, and I hope there is, though I sometimes fear there is not, much of the credit belongs to Mr. Chattock. The only reason why this is not a joint paper is because he had left the laboratory before a result was obtained.

I must mention also that he showed me, somewhere about this same time, an ingenious paper read before the Society of Telegraph Engineers some two or three years back by Mr. George Forbes, in which that gentleman, making free use of the analogy between a magnetic circuit and a voltaic circuit, predicted almost the precise arrangement which we have found successful, as a magnetic analogue of a tangent-galvanometer.

Some other predictions were also made on the strength of the same analogy; but I have made no experiments like them at present.

The experiment of which I have to speak may be described as follows:—You buy at a shop a large hank of iron wire weighing 28 lbs. or so: you cover it with tape and wind it over with No. 14 copper wire connected to a reversing-key and storage battery. At the centre of the ring you place a glass vessel in which are delicately suspended two oppositely-charged very light conductors, made, for instance, of aluminium foil, connected to each other and to a mirror by a shellac arm. You then work the reversing-key by hand in unison with the natural period of vibration of the suspended arm, so as to keep on reversing the magnetism in the ring, and you hope thus to get up a microscopic swing in the arm or to check an already existing one.

Under these circumstances a result may very well be observed; but it will be certainly due to a spurious cause unless far more elaborate precautions are taken.

For a whole day, last Christmas twelvemonth, I got the most beautifully consistent results. The deflexion reversed with the current, with the sign of the static charge, and everything just as it ought. But before jumping to any conclusion the precaution was taken of going through the operations with the key while one end of the wire was disconnected and the battery circuit therefore not complete. The effect was much stronger.

The whole thing was a mere electrostatic effect due to the action of the slope of potential in the conducting-wire on the very sensitive electrometer-like needle.

To get over this effect was very troublesome and seemed impossible. A number of obvious things were tried, such as cutting the wire in the middle and adjusting its two halves symmetrically; putting the middle of the wire to earth, &c. Putting a carefully selected portion of the wire to earth did, indeed, get over the difficulty; but so sensitive was the arrangement that a variation of an inch in the position of the earth-contact on the length of the wire made a perceptible difference. The effect seemed altogether larger than what we wanted to observe; so, at length, I had the whole glass box containing the charged needle coated with tinfoil.

The result was a beautiful steadiness. The slope of potential was utterly cut off and not a trace of oscillation could be detected on open circuit.

But then neither could anything be detected when the circuit was closed. If the needle were at rest, it remained at rest; and if it were in motion, its motion subsided without the least reference to what one happened to be doing with the key. I came sorrowfully to the conclusion that in cutting off the electrostatic disturbance we had neutralized the thing we were looking for.

But on writing this view to Mr. Chattock, I found he did not agree with it. His view was that the magnetic lines had to get through the space in the centre of the coil, and that, though a sheet of tinfoil might delay them momentarily, it could make no difference in the end; since the time required for the reversal of the split-up mass of iron was far less than that of a semi-oscillation of the needle.

Not feeling absolutely sure either way, I determined, at any rate, to try a more sensitive arrangement. Hitherto the suspending-fibre had been horizontal, and the time of oscillation fairly quick, the mirror, as it were, lying on its back, and the light-beam reflected down to it by a shaving-glass.

All this was now changed. The iron ring was set up vertically, and the needle was delicately suspended, care being taken to keep its moment of inertia very small. The ring was wound, not all over, but only over a sector of some 45°, and this wire-covered portion was then carefully screened by sheet copper from being able to see the needle and thereby affect it electrostatically. The copper did not form a closed circuit.

The needle was again a pair of scraps of aluminium-foil attached to the ends of a shellac arm holding also a mirror. The charging was done after a proof-plane fashion, and the insulation was pretty good. Asbestos and sulphuric acid kept the air of the box dry.

Taking suitable precautions and trying the magnetic experiment under the new conditions, with the electrostatic effect of the wire carefully screened off, but no other part of the iron ring screened from the needle, an effect was observed, and it was small. But it was irreversible.

I traced it, after a little difficulty, to Foucault currents set up in the aluminium scraps. One might certainly have expected such currents there; but I did not expect them to matter, except by reason of heat, because the metal is in no magnetic field—except, indeed, the earth's. However, it must be that some few stray lines escape from the iron circuit and, crossing through the air, affect the currents excited in the metal by the changing magnetic induction.

So I had to abandon aluminium, and selected very thin mica, silvered chemically, and held curved to the proper curvature by silk threads. But even in the thin silver film Foucault currents could still be detected; so the film was scored regularly all over with a needle-point. But though this diminished the effect to almost nothing, it was still comparable to what we wanted to observe.

So I beat about for other conductors, good enough to charge statically, but bad enough not to have currents induced in them. At length I hit upon the thin gelatine stuff which is wrapped round crackers. It was not exactly this, but it was some variety of gelatine that Davies ultimately used, constructing it into a pair of little cylinders, say ⅛ inch long by ⅛ inch in diameter, very light, and only just conducting enough to receive a charge after they had been in the artificially dried air of the glass box for some time. The shellac arm and everything were made afresh, and as slight and fragile as possible.

I also proceeded to construct a larger and more carefully made iron ring, instead of a mere hank as bought. I thought of making a figure-of-8 ring, winding one loop with wire, and hanging the needle in the other loop; but practical difficulties postponed the adoption of this plan, a circular ring being so much easier; so I went on with the notion of winding the copper wire only over a sector, and screening that by copper sheet. To make this ring an enormous quantity of fine iron wire was procured, and wound for a great part of a month on a suitable wooden mould in a slow-motion lathe, the wire being passed through a flame and through a stick of shellac on its way, so as to roughly insulate its turns, in case we wanted to employ rapid reversals.

The winding continued until the wooden mould began to break down, when it was extracted, all matted together, and mounted on a stone pillar standing on rock in the laboratory.

Some natural difficulty is found in getting the charged needle to hang as wished, with its mirror in the right aspect. Some difficulty is also found in arranging that the stability of the needle shall not be too much increased by its electric charge, so that it oscillates violently about some point and is not properly slow and sensitive.

Another and very tedious difficulty is to arrange so that the position the needle is willing to take up before and after charge is nearly the same; for if the two be very different the spot of light will not keep still, but will sail steadily along as the charge slowly leaks away. To detect small effects the spot of light must be very still. It was difficult to secure this without at the same time applying too severe a constraint, either electric or other, to the needle. It had to be done pretty much by selecting carefully the shape to be given to the curved plates of the needle, and to the enclosure opposed to them.

All these difficulties were gradually more or less overcome through the patience and skill of my laboratory assistant, Mr. Benjamin Davies.

There remained a few irregular disturbances, some of which could be traced to convection-currents, others to the ordinary movements about a building, and others again to the passage of London and North-Western trains in their tunnel, some 150 yards away and 60 feet down, in the sandstone rock.

The laboratory at Liverpool is very favourably situated as regards shaking. It is a substantial stone building, with walls 2 feet thick, on sandstone rock, which is partially isolated from the railway by the remains of an old quarry, which has been filled with rubble. Street traffic is very distant, and has never been appreciably felt to my knowledge. Students tramping about in the daytime appear to be the cause of most of such irregularities as are felt; but this cause is happily absent at certain hours of the day (or night), and there remains literally nothing but the passage of the North-Western trains, and these are distinctly felt, though only with the most delicate means. The most serious shaking is during a gale. The building then certainly quivers.

To illustrate the unusual steadiness, it may be interesting to mention that Mr. Chattock rigged up the Cavendish Gravitation experiment in my lecture-theatre, taking such precaution as I naturally suggested about suspending from a ball of lead hung from the wall by elastic; and twice I have successfully exhibited the experiment to a fairly large audience, the first time very well indeed. Convection-currents are the real trouble in this experiment, and these can, by elaborate tinfoil screens, be nearly avoided*.

After a few preliminary experiments with the new ring and needle, I began seriously to mistrust the effect of the copper near the ring. Sheet copper was used to screen off the wire from the needle, and it was also used all over the outside of the arrangement to screen off stray electrostatic actions around. But I fancied I understood now what the effect of such conductors was. The motion of the electricity in them was just of the same sign as the motion of the electrostatic charge for which we were looking; and if the motion by conduction were permitted, the reaction of the charge thus momentarily redistributed on the conducting metal might just mask the direct effect of the same electromotive force on the charged body; and would mask it, in so far as it was suitably placed for masking it.

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* Since seeing Mr. Boys's beautiful pocket-arrangement for exhibiting the same effect at the Royal Society soirée this week, I am out of conceit with this apparatus. Evidently the experiment is going to become a commonplace of the class-room.

Metals do not screen off the magnetic effect of a moving electrostatic charge (*vide* Rowland), but they might screen off the converse electrostatic effect of a varying magnetic induction.

On the other hand, by arranging a copper conductor so that the effect of electric charges induced in it by the E.M.F. should *assist* the direct effect of the E.M.F. on the charged body, then no doubt a deflexion might be rendered visible.

So I led a short wire round the outside of the ring, and brought its ends into the box where the needle was, one on each side of the needle.

A distinct and proper deflexion was now observed.

A wire was then taken 10 times round the ring, and connected to a common quadrant electrometer. The deflexion was easy to see. It could also be just seen with only one turn of wire.

This kind of experiment seems likely to furnish a method of determining "v"; and it ought to be a good method, because it is v^2 which is really measured.

However, what I had wanted to see was not any effect assisted by the concentration of a conductor, but a result in free dielectric. And this at length has been obtained.

All copper screens and everything of the sort were removed. The ring was wound with only a few turns of thick wire so as to reduce the slope of potential needed to propel a current through it. The wire was wound symmetrically in two halves, and the far ends were connected to a plug-key, so that one might work with either closed or open circuit.

The reversing-key was a quite distinct mercury arrangement specially made for strong currents, fixed to a table on which stood the screen for receiving the spot of light, which was a small disk of translucent paper in a drum of tin plate mounted on an independent stand. The light of a lamp was brought on to the mirror independently of this stand by a 45° looking-glass. Such a screen is handy, because one can follow the rather erratic spot of light all over the table, wherever it chooses to take up its temporary abode.

The middle of the battery was put to earth, so that the potentials of the two halves of the wire might be equal and opposite. The needle was then adjusted by trial and error, its glass box being pushed in and out of the ring, its suspending fibre being turned by a tangent screw, &c., until the electrostatic effect obtained by working the reverser on open circuit was very small. It could never be got quite zero; and even when small it would not always remain small for long.

However, on now closing the circuit by the plug-key, the residual electrostatic effect would be certainly largely reduced and the wished-for effect might be seen. It was seen; the hair-line of the spot of light oscillated over two or three millimetres when the key was worked in unison with the vibrating arm. It assisted the residue of the electrostatic effect when the charging of the needle was in one direction, and it neutralized or reversed the residual electrostatic effect when the charging of the needle was inverted.

It is this reversal of whatever disturbing effect remained which I depend upon as a proof of the correctness of the observation.

Added later (*May* 10).

More recently Davies has noticed a peculiar disturbance when working on open circuit, and when the residual electrostatic effect is very small; viz., that it is not constant in direction. It begins with a kick one way, and then a larger motion the other way. By working the reversing-key quickly one kind of swing can be got up; by working it slowly, the other kind of swing can be got up.

I do not know the meaning of this effect. It is difficult to believe that the fact that the wire is coiled round iron causes the electrostatic charging of its ends to be slow; though that is the kind of thing it looks like. More probably it is due to some trivial disturbing cause, but the effect is singularly clear and persistent.

To get over the electrostatic effect more completely, and still without the use of a screen, a single layer of copper ribbon was wound on, in about 10 turns, and its outer turn was put to earth.

Another disturbing effect was now noticed: it reversed with the current, and it reversed also when the needle was turned through 180°, but it did not reverse when the charge was simply reversed: nor did it cease when the needle was uncharged. Moreover it was not altogether temporary; there was a residue permanent. Plainly it was a direct magnetic effect.

On taking a map of the field with iron filings, a considerable number of lines of force were found to be leaking across the ring, and these would be able to deflect the needle if it had a trace of permanent magnetization; which it is quite likely to acquire.

It was taken out and tested between the poles of a powerful magnet—the gelatine was distinctly magnetic, the shellac was feebly diamagnetic.

A number of substances were now tried in order to find something sensibly devoid of magnetic properties, and ultimately a paper was found which was barely diamagnetic. With this a new needle was constructed. Wire was once more wound all over the ring, and its field tested with iron filings till hardly any stray lines could be found.

The mirror was removed, and a small pointer supplied instead, so as to be read with a microscope. And in this way the last observations have been made.

The only disturbance now found was due to heat from the wire below the glass box containing the needle, and to get over this Davies rigged up a number of concentric cylindrical jackets of tin plate round the box containing the needle. These certainly constituted an electrostatic screen. Nevertheless, I had hope that though such a screen might diminish the effect it would not destroy it.

I had a letter from Davies yesterday saying that a very minute effect could be seen with the screens all on, and that it did everything properly as regards reversals. On taking off one screen it increased. On taking off another it increased more, but showed signs of superposed spuriousness. On taking off the third screen the effect was masked by spurious actions.

Everything points to the fact, therefore, that we have now several times observed the true effect, and I entertain practically no doubt of it.

But inasmuch as some of the observations have been made so lately, and some even since I left Liverpool this time, I would not wish the Society to suppose that I regard the research as finished and complete. I should like to clear up distinctly the effect of screens.

I must speak in high praise of the skill and neat-fingeredness of my laboratory assistant Mr. Davies in carrying out the rather troublesome requirements which this finnicking research has at various times seemed to necessitate.

Prof. Fitzgerald, in answer to a question from Dr. Lodge as to the influence of screens, said he had not fully considered the matter in this particular case; and as the general effect of screens depended on the square of "v," the subject required careful treatment. As a means of checking the results obtained by Dr. Lodge he suggested calculating the impulse, and seeing whether its magnitude approximately corresponds with that observed. Commenting on an idea for carrying out a similar experiment attributed to him in the paper, in which a charged gold leaf is placed between the poles of a magnet, Prof. Fitzgerald said he had been misunderstood, for he had conceived a disk parallel to the faces of the magnets which, when excited, should cause the disk to turn in its own plane.

Referring to the equations for mechanical force given in Maxwell, § 619, he pointed out that the coefficient of e in the equation $X = cv - bw - e\frac{d\Psi}{dx} - m\frac{d\Omega}{dx}$ ought to be P, where $P = c\dot{y} - b\dot{z} - \frac{dF}{dt} - \frac{d\Psi}{dz}$, and considered it very important that the existence of the term $e\frac{dF}{dt}$ should be tested experimentally.

Prof. S. P. Thompson mentioned some experiments on which he was engaged by which he hoped to show electric displacement in continuous dielectric circuits, such as a hank of gutta-percha. Up to the present the experiments had not been successful, owing to his inability to place the two gramme ring-coils used into such relative positions as to give silence in the telephone connected with the coil used as secondary, when currents were sent through the primary.

Prof. Ayrton suggested that Dr. Thompson's difficulty may arise from the fact that such rings do produce considerable external field even when carefully wound.

Prof. Fitzgerald requested Dr. Thompson to investigate the effects of displacement-currents and of changing vector-potential, and pointed out that in a single medium the former can produce no magnetic effect. As regards fields containing different media, he said the calculations would be complicated by the spurious charges on the separating surfaces.

Dr. Lodge, in reply, said he had calculated the momentum to be expected in one arrangement of his experiment in which a suspended aluminium cylinder surrounds one limb of a rectangular magnetic circuit which formed the core of an induction-coil; one end of the secondary was put to the core and the other to the cylinder, thus forming a condenser. The result came out about 10^{-6} dyne second, but he could not say whether such a small quantity was observable.

XX. *Quartz as an Insulator.* *By* C. V. Boys, *A.R.S.M., F.R.S., Assistant Professor of Physics at the Normal School of Science, South Kensington**.

[Plate II.]

When making quartz threads by the bow-and-arrow process described in the Philosophical Magazine, June 1887 (Proc. Phys. Soc. vol. ix. pp. 8–19), I have sometimes noticed that the thread does not reach all the way from the arrow to the bow, but that the end remains suspended in mid air somewhere between. When this is the case the last foot about, then very fine, is usually in the form of an irregular helix. Under these circumstances, if the hand is brought at all near the end the helix stretches itself out, and the end

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* Read April 13, 1889.

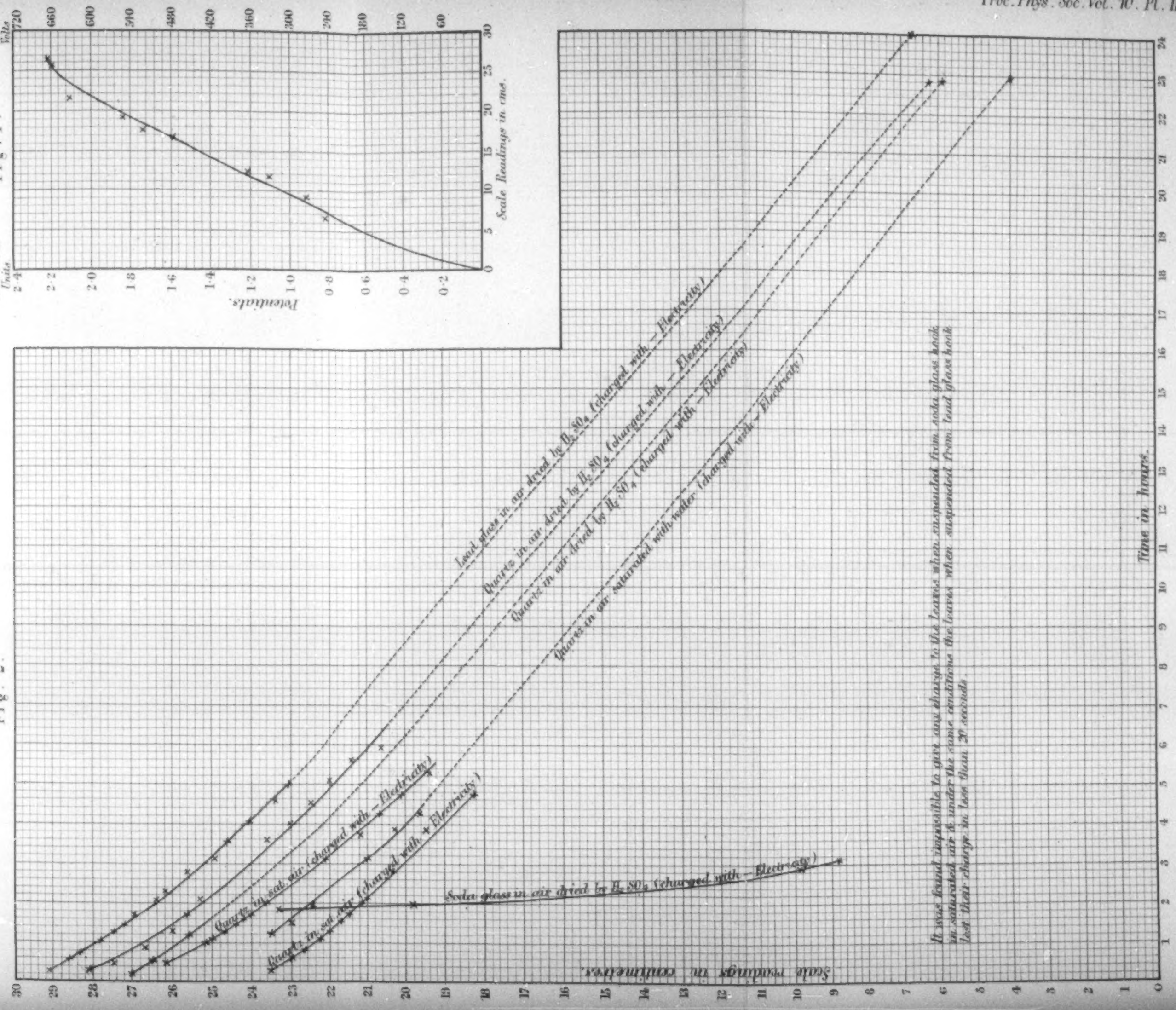


Fig. 1.

Fig. 2.

It was found impossible to give any charge to the leaves when suspended from soda glass hook in saturated air & under the same conditions the leaves when suspended from lead glass hook lost their charge in less than 20 seconds.

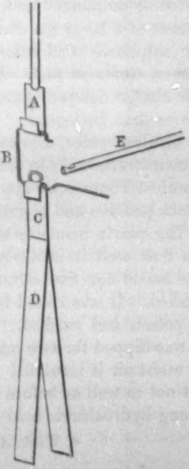
Mintern Bros lith.

of the thread flies to and attaches itself to the hand. On removing the hand the thread takes its old form; and this may be repeated several times.

It did not seem possible to account for this in any way except by supposing the thread to be electrified, though why it should be electrified is not clear. If this is the case then the insulating-power of the thread must be very great, for with the very small quantity of electricity which could remain on a body of such immeasurably small capacity, all trace of charge would escape instantly if the thread insulated no better than glass in the open air.

I therefore thought it would be interesting to see if rods of fused quartz showed any great superiority over similar rods of glass under the same circumstances. The plan that I have followed has been to hang a pair of very narrow gold leaves from the rod under examination, and observe the rate at which they closed after being charged. I purposely avoided all instruments the large capacity of which would increase the time of discharge, and the leakage of which might be comparable with or even exceed that of the rods to be tested.

Fig. 1.



The arrangement of leaves &c. is shown in fig. 1. A flat brass hook, A, is fastened to a rod which can slide stiffly through the centre of the lid of a mahogany box lined with tinfoil. From this is suspended the piece of glass or quartz, B, bent to the form shown, so that it can be handled by the projecting end without touching the portion that acts as the insulator. On this hangs a piece of bent brass, C, to which the leaves, D, are attached. A small ring of wire is soldered to the upper end of C, which is used when one insulator is changed for another as follows:—A stiff wire, E, passes through one side of the instrument, and this is pushed forward through the wire ring. A is then depressed until the lower side of B hangs clear of the hook C. E is then drawn back with C and D suspended from it. Finally, when B is changed, E is pushed forward again, and A raised until the ring on C is just free from E, which is then withdrawn. By this means the leaves are always left at the same level. The leaves are 90 × 5 millim., and the box, which has a glass front and back, is 285 millim. high × 130 millim. wide, and 185 millim. from front to back. The leaves are suspended so that the line of junction of C and D is 160 millim. above the base of the instrument. The length of the insulating portion of the quartz and glass hooks B is 21 millim. and the diameter about 1 millim. The leaves were observed by fixing the object-glass of a telescope at a distance of 393 millim., which projected an image of the leaves on a scale 1940 millim. beyond the lens. Thus the observed divergences were 4·93 times the true distances between the ends of the leaves.

The results of the experiments can be seen from fig. 2 (Plate II.). The rate at which the leaves close is the same with lead-glass in air dried by sulphuric acid, with quartz in air dried by sulphuric acid, and with quartz in air kept moist by means of a large flat dish of water. Soda-glass in air dried by sulphuric acid allowed the electricity to escape about eleven times as fast. With either kind of glass in moist air the charge escaped almost at once, but soda-glass was much worse than lead-glass. The glass had in all cases been boiled in distilled water, a process which Warburg and Ihmori* have shown is necessary in order to make the glass insulate as well as possible. There is no appreciable difference between the rate at which positive and negative electricity escapes from the leaves.

The quartz insulator was then treated in various ways to see how well it is likely to retain its insulating power. It was boiled for five minutes in a weak solution of potash and washed. It was boiled for the same time in a strong solution of potash and washed. In both cases it insulated as before. It was dipped for two minutes in melted potash and washed. In moist air it insulated better than either soda or lead-glass, but not so well as before treatment with potash. Boiling in strong hydrochloric acid did not restore the lost power. A new hook was not affected by boiling in strong hydrochloric acid, or by heating in a batswing gas-flame.

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* Wied. *Ann.* xxvii. p. 481 (1886).

Perhaps the most surprising result is obtained by dipping the quartz hook in water or ammonia, and immediately hanging on the leaves while the water is standing upon the hook in beads. Even so no difference is observed in its insulating-power. If it is dipped in a solution of potash this is not the case; but of course the insulation is restored by washing.

The perfect equality of the rate at which the charge escapes when the leaves are suspended from lead-glass in dry air, or quartz in dry or moist air, makes it probable that this loss of charge is not due to leakage along the insulator, for it is very unlikely that, under these different circumstances, the loss should be exactly alike. It is more probable that the loss is due mainly to convection through the air. This is made certain by the following considerations. The same leaves, when hung by the same hook in another box which was badly made and rough inside, lost their charge much more quickly, but, as before, at the same rate in the three cases. On the other hand the leaves, when suspended in the best instrument by a quartz fibre about ten times as long and one hundredth of the diameter of the piece B (that is, by one which would insulate a thousand times as well if the loss were due to surface creeping, or a hundred thousand times as well if it were due to actual conduction), lost their charge practically at the same rate as before.

Pieces of polished rock-crystal, such as are used as objects for the polariscope, also insulate well; but they do not seem to be quite so free from the influence of moisture as the fused quartz. The same is true with regard to the natural faces and the fractured surface of the crystal.

The electromotive forces required to produce different divergences of the gold leaves were determined by an absolute electrometer, and the results are shown in fig. 1 (Plate II.).

It is probable that this valuable property of quartz, that it insulates perfectly in damp air, may be of use in the construction of electrostatic apparatus. The sulphuric acid now absolutely necessary in electrometers and instruments of that class is nothing short of a nuisance. If the instrument is carried about there is the risk of destruction of the instrument from the spilling of the acid. If the instrument is not moved, the acid, unless specially treated, may give off nitrous fumes which will corrode the surfaces of metal; or, if forgotten, it absorbs water and in time overflows, destroying the whole apparatus. Even if the air were saturated with moisture, rods of quartz would insulate as well as the lead-glass at present used does in air dried by sulphuric acid. The needle should of course be suspended by a fibre of quartz, which is far simpler to apply and adjust than the double line of silk, and superior also in other respects.

In conclusion I must express my obligations to Mr. Briscoe, a student in the laboratory, whose skill in the manipulation of gold leaf and whose suggestions from time to time have been of the greatest service. I have with perfect confidence asked him to carry out the experiments described in this paper, and the results show that the confidence was not misplaced.

XXI. *Water-spray Influence-Machine.*

By George Fuller*.

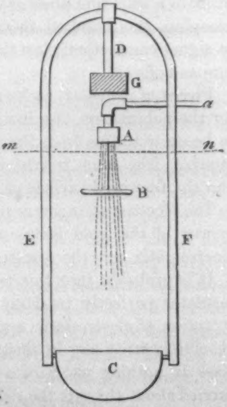
This machine is for obtaining directly from a fall of water a supply of electricity of a high potential. It consists of four similar parts arranged symmetrically round a central vertical support, and each division has the following members.

A nozzle, A, in connexion with a head of water by means of a pipe, *a*.

A ring, B, of brass or copper wire placed vertically below A, and through which the water descends when the machine is in action.

A vessel, C, placed below B to receive the water that has passed through the ring.

A brass tube, E F, between the ends of which the vessel C can turn about a horizontal axis.



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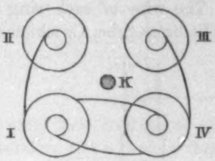
* Read May 25, 1889.

An insulating glass rod, D, to the top of which the tube E F is attached, and with the lower end fixed in a part of the frame of the machine, G.

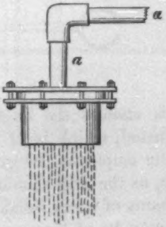
A sectional plan on *m n* shows the connexion between the four divisions, which are numbered I., II., III., IV. The wire ring of section I. is in electrical connexion with the receiver of section IV. Similarly the ring of II. is connected with I., the ring of III. with IV., and that of IV. with I.



K is a central column for supporting four arms of the machine to which are fixed the insulators D.

The discharge of electricity is taken between conductors in connexion with II. and III.

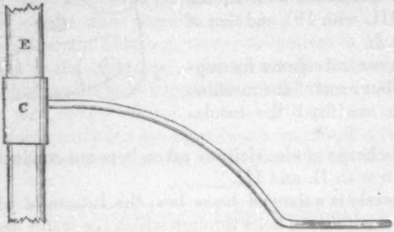
The nozzle is a flanged brass box, the bottom of which is perforated with small holes through which the water descends. It is fixed by a number of small bolts and nuts through its flange to a brass plate fixed to the supply-pipe, *a a*, and the joint is made watertight by a vulcanized india-rubber ring. A piece of fine linen covers the top of the box to strain the water before it reaches the small holes, as it was found that the water was either stopped or diverted by small particles unless this precaution was taken. The holes, which are circular, have a diameter of $\frac{1}{100}''$; as it was found that when holes $\frac{7}{1000}''$ were used the water was so much dispersed by the working of the machine that a great part of the water ceased to fall into the receivers. This great dispersion also injured the insulation, and besides this it was extremely difficult to keep these holes free.

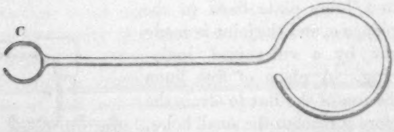


With regard to the number of holes. In the nozzles of sections I. and IV. there are six arranged in a circle of $1\frac{1}{4}''$ diameter. For those of sections II. and III., either a pair with twelve holes each in a circle of 1¼″ diameter, or with eighteen holes in a circle of 1¾″ diameter.

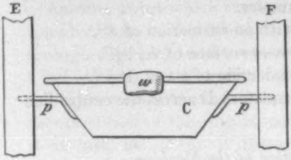
The rings are made of brass or copper wire of about ⅛″ diameter. The inside diameter of the rings used with the nozzles with six and twelve holes is 2¼″, and with the eighteen holes 2¾″.

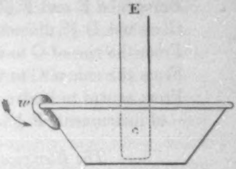
The wire of each ring is continued and fixed to a clip of split brass tube, C, which slides upon the brass tube E or F.



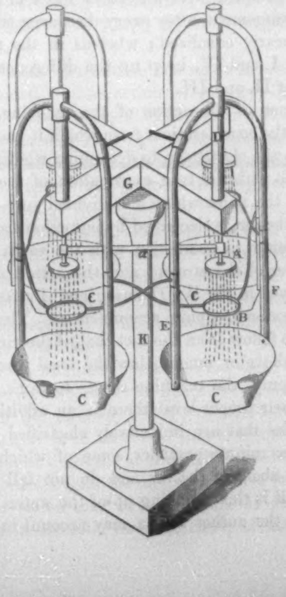


This enables the depth of the ring below the nozzle to be adjusted, which is of importance, as the greater the head of water employed the greater must be the distance between the two, as the ring should be fixed at the point where the small streams of water break up into spray. The receiving vessel, C, may be of glass or metal, as the former material, from its constant state of moisture whilst the machine is working, seems to conduct the electricity as effectually as the latter. In the author's model they were at first of glass, but one of them having been broken they were replaced by receivers of zinc. The receivers are supported by pins, *p*, *p*, which are soldered to them at one end, whilst their free ends rest in holes drilled in E and F. To make the receiver self-acting they are hung so that when a leaden weight, *w*, is fixed, as shown in sketch, the vessels being empty, they would turn in the direction of the arrow if they were not prevented by stops soldered to them which press against E and F; but when





the receivers are nearly full of water, their balance is such that they turn in the opposite direction and so empty themselves. The four receivers are made to turn towards the axis of the machine and to deliver the water into a metal bath, which for continuous action should be connected with a drain.



The following are some of the dimensions of the author's model :—

Zinc receivers 8″ diameter at the top.
Brass tube E and F $\frac{3}{8}''$ diameter.
Glass rod D $\frac{3}{4}''$ diameter, with an insulation of 4″.
From the rim of C to the lower surface of A, $9\frac{1}{4}''$.
From the rim of C to the underside of stand, 1′ 1″.
From centre to centre of insulators D across the centre line of instrument, 1′ 1″.

The Electrical Action of the Machine.

An instrument made of only sections I. and IV., with their rings connected with their receivers, as shown above, will charge itself; and the difference of potential of the two receivers may be such that sparks $\frac{1}{2}$ inch long may occasionally pass between them, though more usually $\frac{3}{8}$ inch is the longest that can be obtained with a head of water of about 23 feet. With this arrangement, after every discharge the potential of the rings is nearly equalized; whereas in the machine with four sections, I. and IV. keep up the difference of potential of the rings of II. and III.

With respect to the action of the machine, the author, whilst giving the considerations from which it was constructed, must leave to the electrician to determine whether they have anything to do with the true explanation of the phenomena. The water, at the point where it is divided into drops by the resistance of the air, is electrified by induction from the rings; the former being in connexion with the earth through the unbroken water of the stream, and the action seems similar to that employed in Sir W. Thomson and Professor Silvanus Thompson's water-dropping accumulators. That such is the case appears to follow from the fact that, if the rings are either placed much above or much below the level where the water breaks into spray, the machine ceases to work. When the rings are at their proper level there is an additional action; for the particles that are inductively electrified are split up into numberless minute particles, some of which are so fine that they float about in the air and do not fall into the receiver. And it is this breaking up of the water into minute particles that the author thinks may account in part for the effect produced; for when a number of spheres that have been electrified unite into a mass of less surface, their potential in the latter state is higher than in the former.

Another point which the author thinks must be taken into consideration is the speed with which the particles move through the ring, as it was only when he experimented with a fall of some feet instead of inches that he obtained a potential high enough to produce sparks. With a very slow speed the attraction of the ring is too strong for the water, so that it at last, as in Sir William Thomson's apparatus, bends against it. That the division of the drops into minute spray plays a part in the action of the machine seems to be shown by the fact that sparks of the same length, in the same state of the atmosphere, have been obtained from it when the velocity of the water has been very much diminished. The sparks, as a rule, have not been so numerous per minute, but the water has been divided into finer spray. At times, even with half the delivery of water, the same length of spark has been obtained.

One experiment the author has made in which the spray was not obtained by the action of gravity, but by a steam "atomiser," as it is called. The water and steam passed through a copper wire ring $1\frac{1}{2}''$ diameter, connected with one of the receivers of an apparatus made up of sections I. and IV., as above. The nozzle was $\frac{5}{8}''$ from the ring and $5\frac{5}{8}''$ from the receiver. Sparks $\frac{1}{4}''$ in length were taken freely from the receiver, which is a better result than has been obtained with a fall of water of some 23 feet. What was very observable in this case was the very small amount of water used, a small teacup-full being passed over in some five or six minutes; and the author has recorded in his notes that the experiment was made on a very wet day.

Adding to the number of jets does not seem to increase the power of the machine, either in quantity or potential, at all in proportion to the number added; though the action of an electrical machine is so eccentric that it is difficult to be certain of this, for at times the nozzle with eighteen jets has given much better results than the one with twelve jets.

It has been stated that, in the machine as made, the rings are $1''$ larger in diameter than the circle of the jets, and it is found that they give a better result than when larger rings are used; but in some experiments with a small flow of water a ring 3½″ diameter gave as large a spark as one of 1½. In the dark, electricity is often seen to fly off from the rings, the water on them being made into pointed-shaped drops.

The machine in its present form is by no means powerful, as with a small Leyden jar attached to it the longest spark has hitherto been 1⅛″, the head of water being about 23 feet. The state of the atmosphere has very great influence on the working of this machine; for though in all states of the weather electricity will be generated, it requires a fairly dry atmosphere to give 1″ sparks.

It may be mentioned that the machine has only been tried in a small bath-room, which is a very unfavourable place for electrical experiments; and it perhaps is worth mentioning, that on one occasion sparks were only obtained when window and door were open and the machine was in a thorough draught.

To what extent the power of the machine may be increased it is difficult to predict; but the author thinks that the experiment with the atomiser points to high velocity in the water, combined with minute subdivision, as the direction in which any future attempts should be made.

Prof. S. P. Thompson enquired whether the length of the spark was limited by leakage along the glass rods or by the spray passing between the receivers, and in reply Mr. Fuller said he thought the former leakage the most important.

XXII. *On the Molecular Weight of Caoutchouc and other Colloid Bodies.* *By* J. H. Gladstone, *Ph.D., F.R.S., and* Walter Hibbert, *F.I.C.*[*]

During the last meeting of the British Association at Bath, we gave a preliminary account of some attempts to determine the molecular weights of caoutchouc and a few other substances by Raoult's method. We have since repeated most of the experiments and largely extended the inquiry, and it seems to us that the results have a certain physical as well as chemical interest.

It is evident that this method is the only one that offers much hope of success in dealing with such substances as caoutchouc, but it is open to question how far the method itself is to be trusted for giving the correct molecular weight of compounds of this description. Our confidence in it, however, was strengthened by the following experiments, made on substances of the same ultimate composition ($nC_{10}H_{16}$), but of known molecular weight in the gaseous condition. We also made experiments on one or two closely allied bodies containing oxygen.

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[*] Read May 25, 1889.

The compounds were dissolved in benzene which had a freezing-point of 5°·25 C., and the experiment was conducted in the usual manner. Each degree of the thermometer scale was divided into twentieths, and it was not difficult to estimate to the hundredth of a degree. Successive observations of a freezing-point nearly always agreed to less than 0°·02.

The following table gives:—in col. II. the recognized molecular formula, in col. III. the strength of solution, in col. IV. the amount of depression, and in col. V. the molecular weight calculated by Raoult's formula $M = \frac{T}{A}$, where T is the molecular depression constant (in this case = 49), and A is the depression given by 1 gram of the substance in 100 grams of solvent. These figures may be compared with col. VI., which gives the molecular weight deduced from the formula in col. II.

Substance.	Col. II.	Col. III.	Col. IV.	Col. V.	Col. VI.
		per cent.	°		
Oil of Turpentine......	$C_{10}H_{16}$	4·56	1·59	140·5	136
Oil of Lemon............	”	6·04	2·17	136·4	136
”	”	3·06	1·12	133·8	”
Cedrene..................	$C_{15}H_{24}$	3·89	1·00	190·8	204
”	”	4·71	1·20	192·3	”
Isoprene	C_5H_8	3·30	2·25	71·9	68
”	”	2·20	1·52	70·9	”
Caoutchene	$C_{10}H_{16}$	5·38	2·01	131·1	136
Heveene..................	$C_{20}H_{32}$	12·00	2·32	275	272
”	”	9·37	1·85	248	”
”	”	7·68	1·53	246	”
Camphor	$C_{10}H_{16}O$	4·69	1·59	144·5	152
Menthol..................	$C_{10}H_{20}O$	3·21	0·93	169·1	156
”	”	4·93	1·31	184·4	”
”	”	3·75	1·07	171·7	”
Anethol	$C_{10}H_{12}O$	3·71	1·29	141·0	148

This table shows not merely that the method is applicable in the case of bodies of this description, but that the molecular weights of the liquids in solution have the same relative values as in the gaseous condition.

We then made experiments on caoutchouc, whose empirical formula as usually given ($C_{10}H_{16}$) would indicate a molecular weight of 136, and we found that this was very far below that deduced from our results, as shown in the following table:—

Substance.	Weight in 100 grams of Solvent.	Depression.	Molecular Weight.
Caoutchouc (*a*) ...	3·1	Scarcely observable.	Extremely high.
„ (*b*) ...	8·8	„	„
„ (*c*) ...	14·6	0·11	6504

The caoutchouc used in solution (*a*) had been prepared from Penang rubber, by the process described in our previous paper (Chem. Soc. Journ., July 1888, p. 679). That in solution (*b*) was obtained from Para rubber, by dissolving it in ether, and precipitating the etherial solution with alcohol. Solution (*c*) was prepared from (*b*) by evaporation in a current of hydrogen. The greater depression observed can hardly be ascribed solely to the greater strength of the solution, since that would only give a proportionate effect. We are inclined to think it possible that there was a lowering of the molecular weight during a three days' gentle heating which was incidentally necessary. The observation, in fact, seems in harmony with other alterations in physical properties which we have sometimes noticed.

This very high molecular weight for caoutchouc strengthens a previous impression of ours that caoutchouc belongs to the class of substances known as colloids. The impression arose from the fact that caoutchouc is a substance showing not the least tendency to crystallize, which cannot be distilled without decomposition, which is subject to great alteration of properties by the action of heat, which is converted into an insoluble modification by small quantities of certain reagents, and which dissolves in its solvents in an extremely sluggish manner.

Graham, in his classic memoir on the subject* of Colloids, observed that "the equivalent of a colloid appears to be always high;" and he also suggested that the colloid molecule may be "constituted by the grouping together of a number of crystalloid molecules."

It seemed worth while therefore to examine bodies commonly regarded as colloidal by Raoult's method. The following table gives the results obtained with aqueous solutions of organic colloids, the molecular weights being reckoned for the ordinary value for T given by Raoult in the case of water :—

Substance.	Weight in 100 grams of Solvent.	Depression.	Molecular Weight. T=19.
Gum arabic	31·6	0°·3	2001
Ditto purified ...	14·0	0·165	1612
Caramel............	8·76	0·105	1585
"	22·5	0·245	1745
Albumen	2·0	Scarcely observable.	Extremely high.

The molecular weight of these known colloids, as determined by Raoult's method, is very high and confirms the generalization of Graham.

Experiments have already been made upon the so-called carbohydrates by this process by Messrs. H. T. Brown and G. H. Morris†. They found that the sugars had a molecular weight agreeing with the received formula, but the noncrystallizable bodies like soluble starch &c. gave them results suggestive of specially high molecular weight.

We may also note that in some recent investigations by C. Lüdeking, he found that the addition of colloids to water makes no practical difference to the boiling-point, and in every case lowers the vapour-pressure very slightly‡. These results all indicate the same general conclusion.

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* Phil. Trans. 1861, pp. 183-224.

† Chem. Soc. Journ. 1888.

‡ *Ann. Phys. Chem.* [2] xxxv. pp. 552-557.

Our experiments were extended by making an examination of solutions of the colloidal hydrates of aluminium and iron. They were prepared by dialysing solutions of the basic chlorides, but, as is well known, a small proportion of the salt must be retained in order to prevent coagulation. The iron solutions contained almost exactly one molecule of chloride to fifteen molecules of the hydrate. The first aluminium solution contained one molecule of the chloride to five or six of the hydrate, the second one of chloride to nearly ten of the hydrate.

Substance.	Weight in 100 grams of Solvent.	Depression.	Molecular Weight. T=47.
Ferric Hydrate	1·16	About 0·01	5452
„	2·60	0·025	4888
Aluminic Hydrate...	0·523	0·060	409·6
„	1·37	0·06	1073·0

The figures here given for the molecular weights of the hydrates are calculated as if the whole depression were due to the hydrate in solution, but the chloride present must have exercised a considerable influence, especially in the first aluminium solution. If allowance be made for this, the molecular weights found would be higher than those given in the table, and would point to the soluble colloidal hydrates of iron and aluminium being many multiples of $Fe_2H_6O_6$, or $Al_2H_6O_6$, which would give a molecular weight of only 214 and 157 respectively. The molecular weights of ferric and aluminic chlorides, as determined by Raoult's method (T being 47), are about 114 and 106 respectively.

All our experiments, therefore, while affording additional illustrations of the value of Raoult's method, confirm the belief that the molecule of a colloidal substance is an aggregate of a very great number of atoms*.

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* Since this paper was read we have found that Paternò and Nasini have arrived at the same conclusion from experiments on albumen and gelatine (*Lincei*, April 7, 1889, p. 476).

XXIII. *On Electric Radiation and its Concentration by Lenses.*

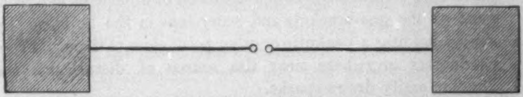
By Prof. Oliver J. Lodge, *D.Sc., F.R.S., and* James L. Howard, *D.Sc.*[*]

Introduction by Dr. Lodge.

In making exact optical experiments on electric radiation it is necessary to be able to converge it and throw a beam of it in any desired direction. To do this by means of mirrors is possible, but not always very convenient. Prof. Fitzgerald and Mr. Trouton[†] have related the difficulty they at first found in making concave mirrors work; and we experienced the same difficulty, intensified probably in our case by the fact that we tried to work with everything on an extra small scale—half the linear dimensions of Hertz.

It is much easier to work with a large oscillator than a small one, because the same extraordinary suddenness in starting the oscillations is not then essential; only with large waves, mirrors and everything have to be heroic to match, and our laboratory was not big enough for optical experiments on gigantic waves. Electrical experiments on such waves I have made in large numbers, obtaining them originally by means of discharging Leyden jars, but recently sometimes by a gigantic Hertz oscillator consisting of a pair of copper plates, each consisting of a couple of commercial sheets soldered together and rimmed round with wire, connected by a length of No. 0 copper wire interrupted in the middle by a couple of large knobs. The plates and connecting-rod are hung from a high gallery, so that everything occupies one plane, their distance and dimensions being here shown.

Fig. 1.—Large Oscillator used for violent and distant effects. Scale $\frac{1}{50}$.



Plates 120 centim. square. Knobs 3·2 centim. diameter.
Each rod 230 centim. long and 8 millim. diameter.
Spark-gap about 1·5 centim.

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[*] Read May 11, 1889.

[†] 'Nature,' vol. xxxix. p. 391.

Static capacity, $\frac{S}{K}$ =25 centim.

Self-induction, $\frac{L}{\mu}$ =8320 „

Characteristic factor, $\log \frac{4l}{d} = 7 \cdot 9$.

Rate of vibration, 10 million per second.
Wave-length, 29 metres.
Dissipation-resistance, 22,500 ohms.
Initial stock of energy, about 300,000 ergs.
Power of initial radiation, 128 horse-power.
Number of vibrations before energy would be *at this rate* dissipated, about 3.

The electrical surgings obtained while the Hertz oscillator is working are of just the same character as are noticed when a Leyden jar is discharging round an extensive circuit; but whereas from a closed circuit the intensity of the radiation will vary as the inverse cube of the distance as soon as the circuit subtends a small angle, the radiation from a linear or axial oscillator varies in its equatorial plane only as the inverse distance, as Hertz showed.

Hence, for obtaining distant effects the linear oscillator is vastly superior. Its emission of plane-polarized, instead of circularly-polarized, radiation is also convenient.

(I may mention that a thundercloud and earth joined by a lightning-rod or by a disruptive path constitute a linear oscillator; and hence radiation-effects and induced surgings may be expected to occur at very considerable distances from a lightning-flash.)

Exciting this oscillator by a very large induction-coil, extraordinary surgings are experienced in all parts of the building, and sparks can be drawn from any hotwater-pipe or other long conductor, whether insulated or otherwise, and from most of the gas-brackets and water-taps in the building, by simply holding a penknife or other point close to them. From conductors anywhere near the source of disturbance the knuckle easily draws sparks.

Out of doors some wire fencing gave off sparks, and an iron-roofed shed experienced disturbances which were easily detected when a telephone-terminal was joined to it, the other terminal being lightly earthed. [Sometimes I utilized the wire fencing as one of the plates of the oscillator, and thus got still bigger and further spreading waves.]

The waves thus excited are from 30 to 100 yards long, and optical experiments with them would be as difficult and vague as are experiments on sound-waves of corresponding length. Small oscillators can, however, easily be employed which shall give waves from a foot to a yard in length; and after reading Hertz's experiment of the pitch prism *, I made preparations for casting some great lenses that should give, I hoped, easy concentration of such waves.

Paraffin was a natural substance to use; but it is rather expensive, and has not a very high index. After considering many substances—beeswax, sulphur, &c.—I decided to try resin, and laid in a stock of that substance. Meanwhile, to gain experience in casting, and finding that a common class of pitch could be obtained at an absurdly low price, I procured several casks of the commonest pitch also. I did not contemplate using this substance at first because I feared it would be an imperfect insulator, and there seemed no use in permitting any dissipation of energy whatever, so long as one could get perfectly transparent substances.

On casting a specimen of the pitch, however, it was found so strongly insulating as nearly to fling off the leaves of a gold-leaf electroscope it was brought near. It seems, therefore, an excellent cheap stuff for electrophorus and such like use, wherever it is not expected to be strictly solid; and it can hardly help being transparent except to very little waves.

Meanwhile we had calculated that to receive rays from one point and convey them to another without aberration, a pair of plano-hyperbolic lenses were very suitable; a parallel beam being transmitted from one lens to the other. The lenses would naturally be made cylindrical, instead of spherical, to suit the linear form of radiator.

The optical calculation of a lens free from aberration for one special point, S, from which it is to receive rays and emit

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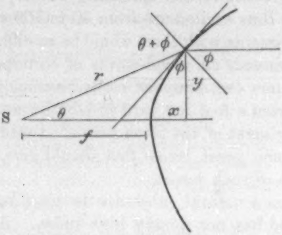
* Wied. *Ann.* xxxvi. p. 769 (1889); translated in Phil. Mag. April 1889.

Fig. 2.



them parallel, is as follows :—From fig. 2,

$$\mu = \frac{\sin(\theta+\phi)}{\sin\phi}\ ; \quad y = (x+f)\tan\theta\ ; \quad \frac{dy}{dx} = \cot\phi.$$

Solving these equations, and making θ and x vanish together, we get, as the curve of the lens,

$$r = (x+f)\sec\theta$$

or

$$r = \frac{f(\mu-1)}{\mu\cos\theta-1}\ ;$$

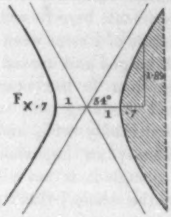
a hyperbola with one focus as origin, with eccentricity μ, semi-latus rectum $f(\mu-1)$, and semi-axis major $\frac{f}{1+\mu}$.

Taking μ as 1·7 for pitch (according to the measurement of Hertz with a prism), and calling the semi-axis major unity, the focal length of the lens is 2·7, the semi-latus rectum 1·89, and half the angle between the asymptotes, being $\cos^{-1}\frac{1}{\mu}$, is 54°.

Using these data, and taking six inches as unit of length, a curve was drawn as shown in fig. 3, where F, the focus of the hyperbola, is to be also the principal focus of the lens ; its distance from the lens is 41 centim.

This curve was given to the laboratory assistant, Mr. Davies, who cut out a pair of wooden templates to the pattern, nailed sheet zinc to them so as to make a mould, propped it up in an outhouse, and proceeded to cast it full of pitch—the upper fluid surface constituting the plane surface of the lens.

Fig. 3.



All went well till the mould was nearly filled, when the weight of the pitch ripped the zinc from its fastenings, and a horrid collapse was the result.

A couple more moulds were made of the same pattern, only stronger, and a bed of sawdust and mould was made to sustain the weight. A double partition of thin wood was introduced across the middle of the mould, so as to enable each lens to be split into two halves if it should happen to be too immovable in one piece.

After a time two satisfactory lenses were obtained, each nearly a metre square. Nothing could be done with them during term, because of want of space; but in the Easter holidays I requested my demonstrator, Dr. Howard, to make experiments in one of the College corridors. There exists a large open room or iron shed, which I should have preferred to use; but unfortunately dry rot had set in in its flooring, and it was in the hands of the carpenters all vacation. We are therefore somewhat troubled by neighbouring walls and by hotwater-piping.

Under more favourable circumstances, the distance between the lenses might no doubt have been much greater; in fact, no attempt was made to place the lenses far apart. They were set up with their flat faces parallel at the opposite ends of a table, about 6 feet apart, and not afterwards moved, being, indeed, rather unwieldy; the oscillator was placed in the principal focus of one lens, viz. at a distance of from 41 to 51 centim. from its curved surface. The focal length, calculated on the assumption that $\mu=1\cdot7$, was 41 centim., but experimentally 51 centim. seemed to do better.

After the few experiments here recorded were done, one of the lenses took advantage of Easter week to assert its essential fluidity, and so much bulged and curved over as to be almost unserviceable; since then it has completed its ruin by breaking its prop and tumbling over into fragments. The other lens stands remarkably well, and seems as good as ever. There is evidently an important difference in the quality of the pitch, though it is not a difference recognized by the invoice. On the whole I think paraffin would have been the best substance to use.

The particular form of receiver is a comparatively unimportant matter, but I prefer linear ones to circular or nearly closed circuits as being more sensitive at great distances, for much the same reason as has been stated for oscillators.

Exact timing of the receiver is unessential. If resonance occurred to any extent, so that the combined influences of a large number of vibrations were really accumulated, the effects might doubtless be great; but hitherto I have seen no evidence of this with linear oscillators; the reason being, I suppose, that the damping out of the vibrations is so vigorous that all oscillations after the first one or two are comparatively insignificant; and very bad adjustment, or no adjustment at all, will give you the benefit of all the resonance you can get from such rapidly decaying amplitudes. The main reason of the rapid damping is loss of energy by radiation. The "power" of the radiation while it lasts is enormous, and the stock of energy in a linear oscillator is but small.

Leyden-jar discharges in closed circuits die away more slowly, and for them some approach to exact timing is essential, if a neighbouring circuit is to respond easily.

In working with small oscillators it is essential that the spark-knobs shall be in a state of high polish, else the sparks will not be sufficiently sudden to give the necessary impetus to the electrification of the conductors.

Any hesitation or delay about the spark permits the potentials of the knobs to be equalized by a gradual subsidence which is followed by no recoil, just as a tilted beer-barrel may be let down gently without stirring up the sediment by waves. The period of a natural vibration is comparable to the time taken by light to travel a small multiple of the length of the oscillator, and hence not a trace of delay is permissible in the discharge of a small conductor if any oscillations are to be excited by means of it. Thus if an electrostatic charge on a conducting sphere be disturbed in any sudden way, it can oscillate to and fro in the time taken by light to travel 1·4 times the diameter of the sphere, as calculated by Prof. J. J. Thomson; and hence it is by no means easy to disturb a charge on a sphere of moderate size except in what it is able to treat as a very leisurely manner. Even on large spheres the oscillations cannot be considered slow: thus an electrostatic charge on the whole earth would surge to and fro 17 times a second. On the sun an electric swing lasts 6½ seconds. Such a swing as this would emit waves 19×10^5 kilometres or twelve hundred thousand miles long, which, travelling with the velocity of light, could easily disturb magnetic needles* and produce auroral effects, just as smaller waves produce sparks in gilt wall-paper, or as the still smaller waves of Hertz produce sparks in his little resonators, or, once more, as the waves emitted by electrostatically charged vibrating atoms excite corresponding vibrations in our retina. It may be worth while to suspend at Kew a compass-needle with a natural period of swing of 6·6 seconds, and see whether it resounds to solar impulses. Another, but almost microscopic, recording needle with a period of $\frac{1}{17}$ second might also be suspended.

The charge on the oscillator used in the present set of experiments vibrates 300 million times a second, which, though slower than the electric quiverings on, say, a three-inch ball, is yet quick enough to demand care and attention.

With very large oscillators, such as that described at the beginning of this paper, no such minute precautions need be taken.

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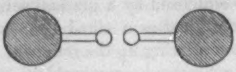
* *Cf.* Mr. Oliver Heaviside, Phil. Mag. February, 1888, p. 152.

Fig. 4.—Small Oscillator used for optical experiments. Scale ⅛.



Plates 8 centim. diameter.
Knobs 2 centim. diameter.
Each rod 6 centim. long and 1 centim. diameter.
Spark-gap about 8 millim.

$$\text{Static capacity, } \frac{S}{K} = 1{\cdot}4 \text{ centim.}$$

$$\text{Self-induction, } \frac{L}{\mu} = 190 \quad ,,$$

$$\text{Characteristic factor, } \log \frac{4l}{d} = 4{\cdot}5.$$

Rate of vibration, 300 million per second.
Wave-length, 1 metre.
Dissipation-resistance, 7250 ohms.
Initial stock of energy, about 5400 ergs.
Power of initial radiation, 128 horse-power.
Number of vibrations before energy would be *at this rate* dissipated, about 1½.

My oscillator is a good deal dumpier, and its ends have more capacity, than those of corresponding wave-length used by Hertz; the reason being that I prefer to make the electrostatic capacity bear a fair relation to the electromagnetic inertia, so as to gain a reasonable supply of initial energy for radiation. The store of energy is proportional to the capacity; the rate at which it is radiated per second is independent of it. Large terminal capacity helps to preserve a high potential longer, and so prolongs the duration of the discharge.

The wave-length of the emitted radiation is easily calculated approximately from the expression

$$\lambda = 2\pi \sqrt{\left(\frac{L}{\mu} \cdot \frac{S}{K}\right)},$$

where $\frac{L}{\mu} = 2l \log \frac{4l}{d}$; l being the length of the entire rod portion of the oscillator, and d its diameter*. The measurement of l is the most unsatisfactory part. It is best to include the knobs and spark-gap as part of the whole length; the constriction at the spark will increase that part of the self-induction, but the expanse of the knobs will diminish another part. A trifle extra length should be allowed for the currents in the disks or balls at the end; but to measure l from centre to centre is rather too much allowance. From centre of one to nearest point of the other is a fair compromise.

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* See Addendum at end.

As to S, it will be practically half* the static capacity of the sphere or plate at either end of the oscillator, especially if these are pretty big compared with the size of the rod. Strictly speaking they are not isolated, even when far from other conductors, because they are in presence of each other, but the correction is usually small. For instance, for two oppositely charged spheres of radius r, at a considerable distance l from centre to centre, the capacity is about

$$\frac{\frac{1}{2}lr}{l-r}=\frac{1}{2}r\left(1+\frac{r}{l}\right).$$

Hence the ordinary value of the capacity, as recorded for convenience below, is always a minimum which circumstances may increase but hardly diminish.

Values of $\frac{S}{K}$ *for Isolated Bodies.*

For a globe,	its radius.
For a thin circular disk,	$\frac{2}{\pi}$ times its radius.
For a thin square disk,	1·13 times inscribed circular disk, or ·36 times a side of the square.
For a thin oblong disk,	a trifle greater than a square of the same area.

Intensity of the Radiation.—Hertz has shown † that the amount of energy lost per half swing, by a radiator of length l charged with quantities $+Q$ and $-Q$ at its ends respectively, is

$$\frac{\pi^4 Q^2 l^2}{3K(\frac{1}{2}\lambda)^3}.$$

He omits the dielectric constant K, because he supposes Q expressed in electrostatic units, but it is better to make expressions independent of arbitrary conventions.

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* *Half*, because the two spheres are technically "in series." See Addendum at end of paper.

† Wied. *Ann.* January 1889; or Nature, vol. xxxix. p. 452.

So the loss of energy per second, being $\frac{v}{\frac{1}{2}\lambda}$ times the above, is

$$H=\frac{16\pi^4(Ql)^2v}{3K\lambda^4};$$

and this therefore is the radiation power.

For a given electric moment, Ql, the radiation intensity varies therefore as the fourth power of the frequency, *i. e.* inversely as the fourth power of the linear dimensions of the oscillator, as Fitzgerald some time ago pointed out.

But inasmuch as different oscillators will not naturally be charged to the same electric moment, but will rather be charged to something like the same initial difference of potential, as fixed by the sparking interval between their knobs, it will be better to write $Q=SV$, and to insert the full expression for λ.

Doing so, we get for the radiation activity at any instant when the maximum difference of potentials at the terminals is V,

$$H=\frac{\pi^4S^2V^2l^2v}{3\pi^4KS^2L^2v^4}=\frac{V^2}{3K\mu^2v^3\left(2\log\frac{4l}{d}\right)^2}$$

$$=\frac{V^2Kv}{12\left(\log\frac{4l}{d}\right)^2}=\frac{V^2}{12\mu v\left(\log\frac{4l}{d}\right)^2};$$

an expression roughly almost independent of the size of the oscillator. Quite independent of it if the length and thickness of its rod portion are increased proportionately.

(The factor μv may always be interpreted as 30 ohms whenever convenient.)

Thus all oscillators, large and small, started at the same potential, radiate energy at approximately the same rate; short stout ones a little the fastest.

But the initial energy of small oscillators being small, of course a much greater proportional effect is produced in them, and the radiation ceases almost instantaneously, their energy being dissipated in a very few vibrations. On the other hand, oscillators of considerable capacity keep on much longer; and with very large ends, as in Leyden jars, the loss of energy by radiation is often but a small fraction of that turned into heat by the frictional resistance of the circuit.

The expression for the radiating power may be compared either with the form $\frac{1}{2}SV^2$ or with the form $\frac{V^2}{R}$; and the loss of energy may be said to be like a static capacity of

$$\frac{30 \text{ earth quadrants}}{6\left(\log \frac{4l}{d}\right)^2}, \text{ or } \frac{5556 \text{ microfarads}}{\left(\log \frac{4l}{d}\right)^2},$$

charged to the potential V, being discharged once a second; or like the heat produced per second in a resistance of $360\left(\log \frac{4l}{d}\right)^2$ ohms, having a difference of potential V between its ends. The duration of the discharge must therefore be exactly comparable to the time a wire of this resistance would take to equalize the potential of the oscillator-ends initially charged to the same difference of potential.

For the small oscillator used in the optical experiments here recorded, the value of $\log \frac{4l}{d}$ is approximately $4\frac{1}{2}$; hence the equivalent resistance is 7250 ohms. And, since the initial difference of potential is, say, 26,400 volts, the power of the initial radiation is 96,000 watts or 128 horse-power.

At this rate the whole original stock of energy (5400 ergs) would be gone in the two-hundred millionth of a second, *i. e.* in the time of $1\frac{1}{2}$ vibration; but of course the energy really decays logarithmically. The difference of potential at any instant being given by

$$\frac{d(\frac{1}{2}SV^2)}{dt} = \frac{V^2}{R}, \quad \text{that is, } V = V_0 e^{-\frac{t}{RS}};$$

where R is the above 7250 ohms plus the resistance of the spark and of the oscillator itself to these currents. The resistance of the spark is probably but a dozen, or perhaps a hundred, ohms; that of the small oscillator is about $\sqrt{(lr)}$ ohms, where r is its ordinary resistance to steady currents expressed in ohms, and l is its length in centimetres. This, therefore, is utterly negligible; practically the whole of its energy goes in radiation. For the big oscillator the resistance is about $\sqrt{(\frac{1}{30}lr)}$; and so for a linear oscillator in general the dissipation resistance may be considered as simply

$$R = 360\left(\log\frac{4l}{d}\right)^2 \text{ ohms.}$$

Nothing approaching continuous radiation can be maintained at this enormous intensity without the expenditure of great power, a hundred and thirty horse-power if my calculation is right. Under ordinary circumstances of excitation the intervals of darkness are enormous; if they could be dispensed with, some singular effects must occur. To try and make the radiation more continuous a large induction-coil excited by an alternating machine of very high frequency, or by a shrill spring-break, might be tried. But even if sparks were made to succeed one another at the rate of 1000 per second, the effect of each would have died out long before the next one came. It would be something like plucking a wooden spring which, after making 3 or 4 vibrations, should come to rest in about two seconds, and repeating the operation of plucking regularly once every two days.

Statement of Results by Dr. Howard.

The apparatus used consisted of (1) an oscillator, or transmitter, with exciting coil; (2) a resonator or receiver, and (3) two lenses of pitch. We shall describe these in order.

The Oscillator or *Source of Radiation.*—This was made in two similar halves, each constructed by soldering to one end of a brass rod, 6 centim. long and ·95 centim. diameter, a thin circular copper disk of 4 centim. radius. To the other end was soldered a spherical brass knob of 1 centim. radius, highly polished. A small hole was drilled in each rod at a distance of 1·3 centim. from the knob to allow of the insertion of connecting wires to the Ruhmkorff coil by which it was excited. The two disks were cemented to two small wooden blocks which could be clamped in any position on a vertical glass rod. By this means the distance between the knobs could be easily adjusted, and the apparatus could be inclined when wished.

The induction-coil was of the usual pattern with hammerbreak. With the current used (supplied by 6 accumulator-cells) it gave a continuous stream of sparks between two points 2·5 centim. apart connected to the secondary terminals. The knobs of the oscillator were usually separated by a space of from ·7 to 1·0 centim. They required cleaning about every 20 minutes owing to burning produced by the spark. This burning was always greater at one knob than the other; greatest apparently at the one that mattered least, for if the primary current was reversed after the oscillator had been working some time the intensity of its radiations immediately decreased perceptibly.

The length of a *complete* wave emitted by the oscillator, calculated from its dimensions after the manner of Hertz, is 100 centim. And this is a sufficient amount longer than the conductor itself for the calculation to be not very inexact. It cannot pretend to accuracy.

The Resonator, or receiver and detector of radiation (the electric eye, as Sir W. Thomson calls it), was of the simplest possible construction. Two pieces of copper wire (No. 13 B. W. G.) were cut each to a length of 25 centim. One end of each was rounded off, and to the other end was attached a small rectangular brass scrap or plate at right angles to the wire. These little plates each carried a point; one of these points was fixed, and the other adjustable by a screw, by means of which the distance between them could be varied. The resonator was fastened to a piece of wood a little longer than itself. Its total length, including points and strips, was 53 centim., *i. e.* about half the calculated wave-length of the oscillator. A better mode of expressing it is to say that each half of the resonator is approximately a quarter wave-length, and corresponds to a closed organ-pipe, or to a resonant column of air in a glass jar.

The lenses were made of common mineral pitch, which was found to insulate quite well enough for the purpose. They were cast in the form of hyperbolic cylinders, bounded by a plane perpendicular to the axes of the principal hyperbolic sections; the eccentricity of the latter was equal to 1·7, and was taken as a fair approximation to the refractive index of pitch for infinitely long waves. A lens of this form should converge a bundle of parallel rays falling normally on its plane surface to a line of foci coinciding with the outer foci of its principal hyperbolic sections; and, *vice versâ*, rays proceeding from this focal line and falling on the curved surface should emerge from the lens as a bundle of parallel rays. Hence, if the oscillator be placed along the focal line of one lens, the electric rays from it will be sensibly parallel after traversing the lens, and after falling normally on the plane surface of the second lens should converge and meet at its focal line. The lenses were almost equal in size. Their plane surfaces were nearly square, being 85 centim. high and about 90 centim. broad. The greatest thickness (from vertex of hyperbola to plane surface of lens) is 21 centim. The lenses are each separated into an upper and lower half by means of a thin wooden partition inserted during the casting. It was intended to divide this partition by a saw-cut, and thus allow the lenses (each of which weighs more than 3 cwt.) to be more easily carried about. So far, however, this has not been done.

In making the experiments the lenses were placed one at each end of a wooden table 2½ metres long, with their plane surfaces turned towards each other, and as nearly as possible parallel. The distance between them was 180 centim., and remained the same throughout the experiments. On one side of the table close to the edges of the lenses was a brick wall about 40 centim. thick; and on the other side was a residue of gangway 54 centim. wide between the lens and a laboratory-apparatus cupboard, which has had to be set up in the corridor for want of space elsewhere. The oscillator stood, together with its exciting coil, on a small table whose height was adjustable; the plane of its disks was parallel to the flat surfaces of the lenses in all cases. It was intended to be placed in the focal line of the first lens; but apparently the index of refraction had been assumed too high, and a position 51 centim. from the vertex of the lens seemed to do best. We shall speak of the vertical plane through the focal lines of the two lenses as the "*axial plane*." It contains the axes of the lenses and of the oscillator. Waves seem to be emitted more powerfully in this plane normal to the disks of the oscillator than in the plane containing them.

The direct effect from the oscillator could be perceived by the resonator at a distance of 120 centim. in the axial plane in the most favourable case; that is to say, in a very dark room and just after cleaning the knobs of the oscillator. Under similar circumstances resonance was only just obtainable at the vertical edge of the first lens, viz. 85 centim. from the oscillator in a direction making an angle of about 30° with the axial plane. To get a rough measure of the intensity of the radiation at any point, the resonator was placed there, and its spark-gap arranged so as to just give a continuous stream of sparks; it was then brought to the line joining the oscillator and the edge of the first lens (line of reference), and the distance from the oscillator observed at which the sparks ceased to be continuous. When the intensity of the radiation was very small, however, the converse of this method was adopted; the resonator was adjusted at the line of reference and then taken to the point at which the intensity was to be observed.

The following are the phenomena observed in the space between the two lenses when the oscillator coincides with the focal line of the first one. The resonator gives brilliant sparks in the axial plane near the first lens so long as it is held parallel to the oscillator. On rotating it in a plane perpendicular to the axial plane the sparks decrease in brilliancy and length, and become entirely obliterated when the resonator and oscillator are at right angles. If the rotation is continued the sparks reappear and regain their former brilliancy, when the resonator again reaches its first position. If the resonator be placed in the axial plane and then moved parallel to itself towards the edge of the lens, the intensity of the sparking gradually decreases as we get nearer the edge, and on the side nearest the wall the sparks cease altogether at the edge of the lens. On the other side, however, they are visible right up to the edge of the lens, and then very abruptly cease, when the direct effect alone is obtained. The beginning of the sparking, as soon as the resonator enters the shadow of the lens, is very noticeable. The same appearances are observed at all distances from the first lens, but the intensity of the radiation is, of course, smaller as we get further from the oscillator. The radiations are always a little more feeble on the side nearest the wall than on the other side. The cause of this has not yet been definitely ascertained, but it appears to be produced by some action of the wall itself. Slightly altering the position of the oscillator did not get rid of the effect; so it cannot be due to the oscillator being out of focus. There was apparently no defect in the lens itself which could account for it. The concentration of the radiations by the lens is very well marked. Just after passing through the first lens in the axial plane they are almost as intense as when they first impinge on its curved surface; that is to say, they do not lose appreciably in intensity by traversing the 21 centim. of pitch. But this concentration is even more clearly shown by the fact that in the axial plane, at the surface of the second lens (250 centim. from the oscillator), the sparks are quite as intense as the direct effect would be at 100 centim. in the same plane if the first lens were removed; or, again, the resonator will give sparks easily at the surface of the second lens, and when brought to the line of reference will not give sparks at a greater distance than 70 centim. At the surface of the second lens the irregularity mentioned above is a little greater than at the surface of the first one.

Beyond the second lens the rays are converged, as we expected, and there is a fairly well defined point in which they meet; but the intensity of the sparking at the focus of the second lens is not appreciably greater than at its surface. Probably this is due to the fact that the rays from the edge of the lens, having travelled a much longer distance in air than those in the axial plane, have thereby lost much of their intensity; and the differences between the intensities at different points could only be detected by a resonator with more delicate adjustments. The cone of rays between the second lens and its focal line is of almost uniform intensity in the neighbourhood of the axial plane. At the edge of the cone the intensity falls off very rapidly; and if the resonator be moved parallel to itself in a plane perpendicular to the axial plane, it shows sharply, by the commencement and stoppage of its sparking, where the boundaries of the cone lie. The cone is a little unsymmetrical on account of disturbance at the side nearest the wall, but the convergence of the rays to a focus is placed beyond a doubt.

The following observations were made on the rays after they had passed the focus of the second lens. The resonator, after having been set to spark at a distance of 80 centim. from the oscillator in the line of reference, was taken to the focus of the second lens, and there gave sparks of fair intensity. Beyond the focus there were traces of a divergence of the cone of rays, which became more evident when the oscillator knobs were quite clean; but in order to make sure of the existence of this divergence a more sensitive resonator would be necessary. In the axial plane itself the resonator used by us gave an effect when the conditions were most favourable, at a distance of 120 centim. beyond the focus of the second lens; and it would possibly have given an effect still further away, had there not been an iron hot-water pipe 9 centim. in diameter running from floor to ceiling of the passage near this point. The furthest point at which any traces of sparking could be found was in one case 450 centim. from the oscillator, while without the lenses it was only 120 centim. This statement has to be taken along with the fact that the lenses were only 180 centim. apart, and that no attempt was made to elongate the parallel portion of the beam by increasing their distance.

In order to determine experimentally the wave-length of the oscillations, a sheet of tin-plate was set up against the flat (inner) surface of the second lens. The rays reflected from this plate were thus made to interfere with those incident on it so as to give stationary waves, as in some experiments of Hertz. The result was that close to the plate there were no traces of sparking. On taking the resonator further away, however, the sparks appeared, reached a maximum, and then disappeared again at a distance of 50 centim. from the plate. The point of disappearance was very definite. The sparks appeared again when the resonator was still further withdrawn, and as long as it was kept parallel to the oscillator no further disappearance of the sparks could be observed. By rotating it, however, in the axial plane, a point was found at which the amount of rotation required to make the sparks disappear was a minimum. In this position the centre of the resonator was 101 centim. from the reflecting plate. The observations agree with the previously calculated value of the wave-length, viz. 100 centim.

In the above experiments the oscillator was always placed in the focal line of the first lens, that is, vertically. Some observations were made later, after turning the oscillator through an angle so as to leave its centre in the axial plane, but its direction inclined to this plane. The effects were always of the same nature as those already described, even when both oscillator and receiver had been turned through a right angle, but the intensity of the radiation was not so great beyond the first lens. The focussing of the rays by the second lens could not be observed in this case, even when they were rotated only ten or twenty degrees, as the intensity was too small.

The above results all go to confirm the identity of electrical radiation and light; and are merely a slight extension of the famous researches of Hertz.

University College, Liverpool,
May 1889.

ADDENDUM *dated June* 20.

An expression for the self-induction of a straight copper rod, of length l and thickness d, we do not see how to calculate on Maxwellian principles without some sort of a return circuit somewhere. On action-at-a-distance principles it can be done thus:—

Consider two parallel filaments or thin straight wires at a distance c apart; call an element of one, at a distance a from some plane of reference, da, and an element of the other, at a distance b from the same plane, db. The mutual induction or potential of two elements on each other is

$$\frac{da\,db\cos\epsilon}{r},$$

where ϵ is the angle, and r the distance, between them. Hence the mutual induction of the two parallel filaments, each of length l, is

$$\mathrm{M}=\int_0^l\int_0^l\frac{da\,db}{\sqrt{\{(a-b)^2+c^2\}}}.$$

Integrating with regard to a, this becomes

$$\mathrm{M}=\int_0^l \log \frac{\sqrt{\{(l-b)^2+c^2\}}+(l-b)}{\sqrt{(b^2+c^2)}-b}\,.\,db,$$

where numerator and denominator are of the same form if the limits for the numerator portion have l subtracted from them both. Performing the integration of the two parts separately, and simplifying, we get

$$\mathrm{M}=2l\log\frac{\sqrt{(l^2+c^2)}+l}{c}-2\{\sqrt{(l^2+c^2)}-c\}.$$

It may be worth while to write down the form this assumes when c is moderately small compared with l, viz.

$$2l\left\{\log\left(\frac{2l}{c}+\frac{c}{2l}\right)-\left(1-\frac{c}{l}+\frac{c^2}{2l^2}\right)\right\}.$$

If we now put for c the geometric mean distance of the points in a cross section of a rod of thickness d, we shall have the mutual induction of the parts of all the filaments in that rod upon each other, *i. e.* the self-induction coefficient of the rod. And unless the rod is very short and thick, it will be permissible to neglect the c/l terms.

Now the geometric mean distance of the points in a circular section varies from $\frac{1}{2}d$, when they are concentrated into its circumference, to $\frac{1}{2}e^{-\frac{1}{4}}d$, or $\cdot 3894\,d$, when they are spread uniformly all over it. The first case corresponds to our rapidly periodic currents, and gives, as the self-induction of a rod in which currents keep to the periphery,

$$\mathrm{L}=2l\left(\log\frac{4l}{d}-1\right);$$

whereas if the currents penetrate all through its section, by reason of being of slowly changing strength,

$$\mathrm{L}=2l\left(\log\frac{4l}{d}-\tfrac{3}{4}\right).$$

The difference is not marked : at least for the case supposed, of non-magnetic material.

Hertz employed this last formula, quoting it apparently from Neumann ; but he says that in Maxwell's theory the $\frac{3}{4}$ turns into $\frac{1}{4}$. We do not know how he makes this out, but suppose he is somehow right ; and it is this uncertainty which has caused us to refrain from going into minutiæ on the subject, and to be satisfied with using merely $\log\frac{4l}{d}$, instead of $\left(\log\frac{4l}{d} - \text{something}\right)$, for what we have called the characteristic factor. It is easy to subtract 1 from it if that is the proper thing to do, as our calculation indicates it is. But the violent constriction at the spark, in the case of an oscillator, must cause a considerable increase of self-induction.

It may be interesting just to quote in similar form the self-induction of the same rod bent into a circle, viz.

$$2l\left(\log\frac{4l}{d} - 2 - \log\frac{\pi}{2}\right),$$

if the currents keep to its periphery. When they penetrate its section uniformly the 4 becomes 5·14, and that is all the change unless it is made of magnetic material.

It thus approaches the same value as the straight rod for infinite length, but is always distinctly less.

There is one point on which we find ourselves differing from Hertz. We regret to say that our calculation of radiation-intensity comes out four times as great as his. We get the same formula as he does, so there is no slip in the working there; but, in the application, a 2 or a $\sqrt{2}$ comes in wrongly in one or other of our calculations. His using half-wave lengths is a natural source of confusion, but we have avoided all that; and it must be that it is owing to a different calculation of the effective capacity concerned in an oscillator that the discrepancy arises. If an oscillator has spheres 30 centim. diameter at either end, Hertz calls its capacity 15 centim.; we call it 7½. We feel bound to call it 7½ according to any method of calculation; although the radius of either sphere is the natural thing to write down at first thought. The charge which surges into either sphere has had to come from the other, not from the earth or anything of infinite capacity. The two spheres are therefore like two condensers in series. Hence our wave-lengths are $1/\sqrt{2}$ of Hertz's wave-lengths (or rather $\sqrt{2}$ times what he *calls* his wave-length); and since λ occurs to the fourth power in radiation intensity, it makes our radiation 4 times as strong for a given oscillator as that which he would calculate. This discrepancy we by no means view lightly, and it is not without many qualms that we find ourselves differing, even about a 2, with a man so splendidly careful in his work as Hertz has shown himself. It is more than probable that he is right after all, so we explain what will then turn out to be our error in this note.

Prof. Fitzgerald congratulated the authors on their success, and also pointed out that, although large oscillators give good results at distances within a few wave-lengths, yet at greater distances small ones were decidedly superior, owing to the energy of radiation varying as the 4th power of the rapidity. He had recently made experiments on electric radiations analogous to Newton's Rings, and had successfully observed the central dark spot and the first dark band. Referring to Dr. Lodge's experiments, he inquired whether any traces of diffraction were observed near the boundary of the bundle of rays between the lenses. Speaking of polarization-experiments, Prof. Fitzgerald said waves reflected from films of water exhibited no polarization, whereas those reflected from nonconductors were completely polarized.

In reply Dr. Lodge said no diffraction-effects had been observed, but in the interference-experiments to determine wave-length the positions of minimum effect were very decided.

XXIV. *An Apparatus Illustrating Crystal Forms.* *By* R. J. ANDERSON, *M.A., M.D., Professor of Natural History in Queen's College, Galway**.

[Plate III.]

THE apparatus by which I propose to illustrate crystal forms consist of frameworks and cords and weights.

The first piece of apparatus is figured in Plate III. fig. 1, and consists of a frame made of wood. This is divided into two compartments. One of these has, above, a slit half an inch wide that runs from end to end; in this slit a slide moves to and fro, and can be fixed by means of a binding-screw at any place.

A slide of a similar kind moves in a slit in the lower part of the framework; this can be fixed by binding-screws in any position. Pulleys are fixed at the ends of this compartment.

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* Read April 13, 1889.

Slips of wood run from end to end at the sides and carry riding-slides. These slides have binding-screws and pulleys whose sheaves revolve on a vertical axis fitted to them.

A figure is easily constructed by carrying cords over the pulleys. Single cords only are shown in the figure. This is for the sake of distinctness.

Starting from α', which marks a ring connected with the weight p, a single cord runs through β' (ring), η' (ring), δ', through ring α', through η', to be fixed to a weight.

A second cord starting from γ' runs through α', ϵ', η', through ring γ', and then across to ϵ' through this ring to hook up a weight p'. A third cord is fixed to δ' and runs through ϵ', β', γ', through δ' and β' to loop up another weight. The actual tension-weights are fixed to the small rings, which act as pulleys.

Each rhombus has in this way a cord to itself, and the size of the rhombus depends on the weights attached. The smaller the rhombus the more cord is to spare.

The figure shown is the regular octahedron if the axes be equal. This condition is easily produced by adjusting the weights.

The octahedron of the second dimetric system, or pyramidal system, is produced by increasing the weights above and below.

The octahedron of the third system may be easily formed by increasing a pair of the horizontal weights.

The octahedral figures may be easily formed by leaving out the diagonals and running the cords from the rings at one extremity of the rhombuses through two rings, and then through the opposite ring, to be there fixed to a weight. The tension-weights, as shown in the figure, will then correspond to the apices of the rhombuses.

For the oblique systems further changes are necessary. The upper slide is moved to the right and the lower to the left, or *vice versâ*. This is attended with elongation of the vertical axes, and the cords passing through the pulleys above and below and at the ends are increased, and the slack below is pulled in to a less extent. The other sides of the octahedron are less affected.

In the first place, the lateral rider-slides are allowed to

Fig. 1.

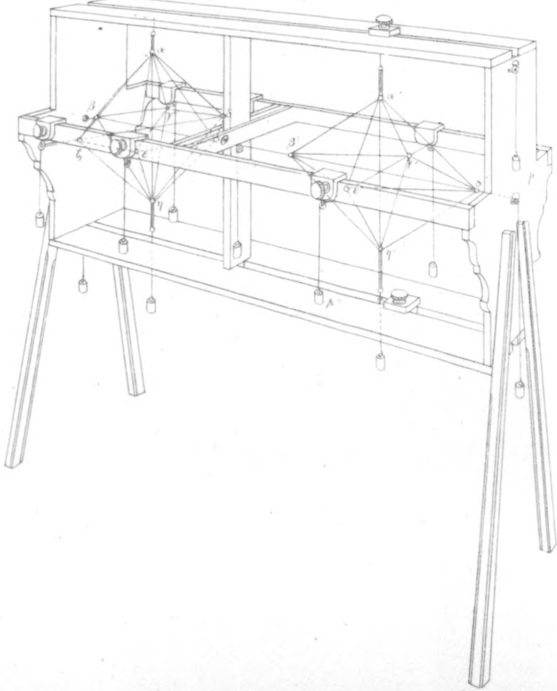


Fig. 2.

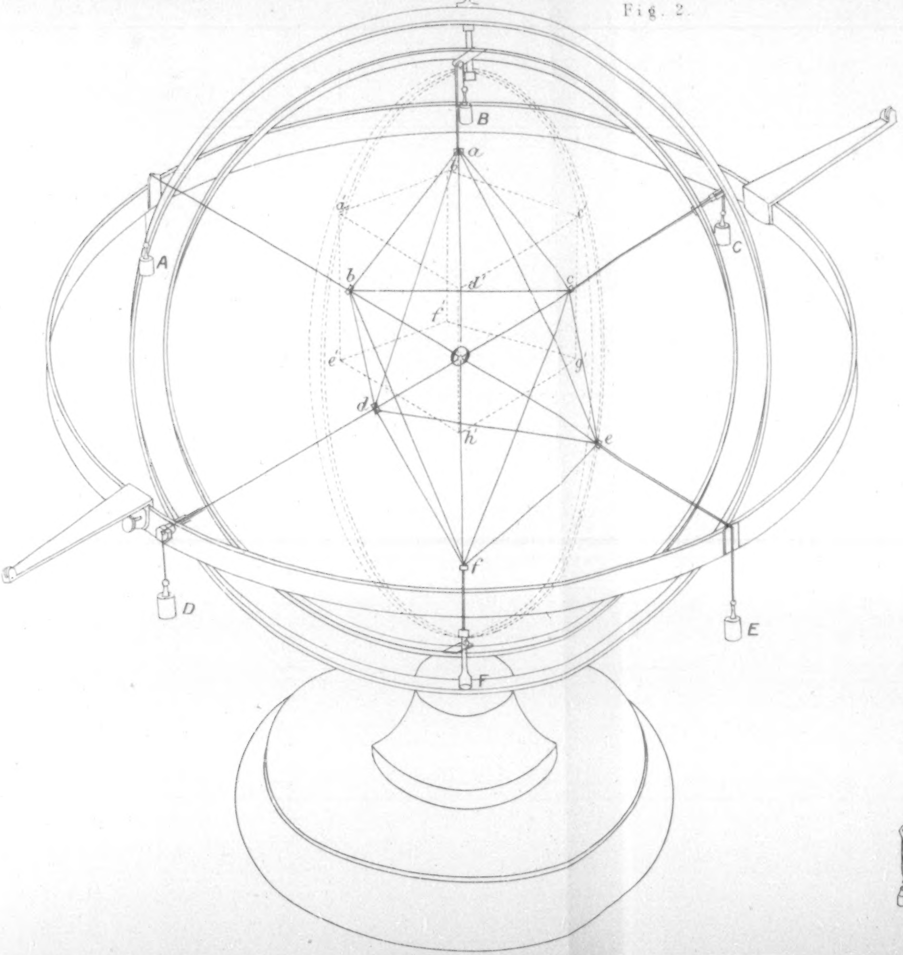
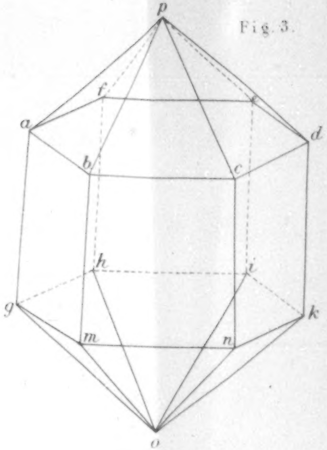
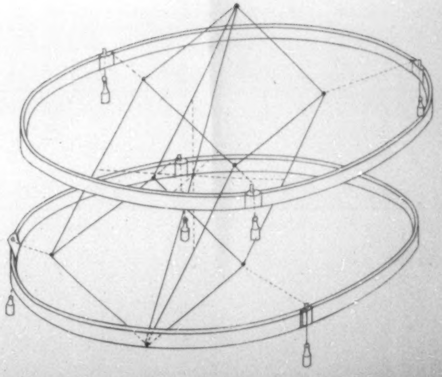


Fig. 3.





Mintern Bros. lith.

remain in a position such that the line joining them is perpendicular to the central vertical longitudinal plane. This gives the Monoclinic System.

Secondly, the rider-slides are moved one to the right, the other to the left, and in this way the *Anorthic* or Triclinic System is produced.

In each case it is desirable to have the slack for each rhombus at different angles of the octahedron.

All the possible varieties of the fifth system cannot be produced in this way. So it is necessary to arrange for the elevation and depression to the rider-slides in extreme cases. This is accomplished by means of a large ring which carries a pulley.

I have chosen the octahedron as the simplest figure.

The cube is formed by the introduction of two horizontal hoops, one above and one below the level of the horizontal bars. These by a simple mechanism are made movable ; and if eight pulleys be fixed opposite the eight edges of the octahedron, and the edges of the octahedron be drawn out by rings running on these cords, it will be necessary, then, only to run cords through rings above and below, and to relax the horizontal and apical weights in order to produce the cube.

The modifications caused by truncating or bevelling the edges or faces can be produced by increasing the number of the hoops or rings. For the simplest figures, however, vertical hoops answer best. The sliding-rings that are carried by the cords may with their transverse cords be lowered to the level of the bar again, and the octahedron again produced.

The cube and the corresponding forms of the pyramidal and prismatic systems may be easily constructed by running the cords as follows :—

Take the cube as

$$\begin{array}{cccc} a' & b' & c' & d' \\ e' & f' & g' & h' \end{array}$$

where the first row represents the upper face, and the second the lower, as in fig. 2.

The cords will have the following course :—

$$\begin{array}{ccccc} b' & g' & h' & f' & b' \\ e' & d' & c' & f' & e' \\ f' & a' & d' & g' & f' \\ c' & h' & e' & b' & c' \end{array} \qquad\qquad \begin{array}{ccccc} a' & b' & g' & h' & a' \\ b' & c' & h' & e' & b' \\ c' & d' & e' & f' & c' \\ h' & d' & b' & f' & h' \\ g' & c' & a' & e' & g' \end{array}$$

The faces of the cube corresponding to the angle of the octahedron.

Now by drawing out the cords opposite the middle of the faces (that is, the diagonals of the faces) a 24-sided figure is produced which can be reduced in the limit to a 12-sided figure, namely the rhombic dodecahedron.

If the sides be connected by cords with pulleys and drawn out, and at the same time cords connecting the centres of the sides with the centres of the faces be drawn out, then the trapezohedron is produced.

Cause the two lateral pulleys of the cube to approach above and the longitudinal pair to approach below, and the tetrahedron is produced. A prism surmounted by pyramids is produced by drawing out the diagonals of the terminal faces; from this the corresponding octahedron may be obtained by a simple method.

The other part of the framework is shown in the drawing (fig. 1, left-hand) as containing the double hexagonal pyramid. Sliding-pulleys, as in the part already described, are fitted above and below. Rider-pulleys are attached to the bars at the sides. Two are shown on each side.

The cords are attached in this way:—A bundle of six are fastened together above to a cord, and drawn by this cord through a ring. The cord passes over two pulleys and reaches a weight outside the framework. The six cords pass through the rings marked β, γ, δ, ζ, θ, and ϵ in the figure, then down to be attached to a cord below, which goes through a pulley-sheaf. The rings marked by the Greek letters are seen attached by cords to weights, through each of these a cord passes. This cord is carried through one of the rings and kept there by means of a small weight. The ratio of the vertical to the horizontal axes may be easily altered by means of the weights. The approximation of the lateral pulley gives rise to the octahedron. The number of the sides may be increased by increasing the cords and pulleys.

In order to show other figures two hoops are fitted to the framework, above and below. The cords of the pyramids are hooked out, and the cords connected with the hooks pass over pulleys and are attached to weights. A cord is made to go through the rings (hook-rings) above and below.

By running down the rings and unhooking the weights above and below, the hexagonal prism is produced.

Prisms with more sides can be produced by increasing the number of the cords which correspond to the edges. The pyramids surmounting the prism are produced by drawing out the cords at the extremities of the prism. Figures with fewer sides are produced by causing the pulleys to approach. Forms the result of bevelling and truncation are produced by pulling out the cords of the terminal pyramids and running other cords through the rings. The original double six-sided prisms are produced by causing the hoops to approach one another.

The ikosahedron is produced by forming the five-faced equilateral pyramid above and below, and approaching the hoops towards one another, so that the distance between the hoops is equal to the perpendicular of one of the triangles. Then it is only necessary to rotate the lower hoop though 36°, and to connect the obtuse angles of the rhombus. In this way the figure can be produced.

The relations of the hexagonal to the rhombohedral division of the sixth system may be shown in this way. Take the double pyramid, hook up each alternate horizontal angle, and hook down the others. Adjust suitably the superior and inferior angles, and the rhombohedron is produced. The cords in reality follow the course of the lines in the glass models.

This method is very interesting in this way, that by a little dodging the rhombohedron can be converted into the cube, so that the relations between the sixth and other systems are rendered more distinctly apparent.

The rhombohedron may be easily changed into the hexahedron by unhooking the weights and pulling in the cord. The hoops are shown in the lower part of fig. 3, Pl. III., with the rhombohedron attached. The hexagonal prism is figured separately for the sake of distinctness.

The ikosahedron may be produced by hooking up and down the horizontal cords of the decahedral pyramids. If we begin with the double octahedral pyramids, the rhombic dodecahedron can be easily produced by hooking up the corresponding alternate edges above and below, and running cords through the hooks looped up and those rings still stationary.

In order to show the effects of uniting and separating forces the form shown at fig. 2 is useful. The instrument consists of a frame in which hoops revolve, some on vertical and others on horizontal axes. The hoops carry sliding-pulleys as shown on the plate. The cube is easily constructed by running cords over eight pulleys fixed on two rings revolving on a vertical axis. Cords are carried through small rings above and below (fig. 2, $a' \, b' \, c' \, d' \, e' \, f' \, g' \, h'$).

Without going into details, it will be easily seen that one orthogonal hexahedron can be easily changed into another, and into the corresponding octahedron. The octahedron of the first system, $a \, b \, c \, d \, e \, f$, if constructed by running cord over the pulley B, and the pulley attached to the same ring below, may be changed into the octahedron of the dimetric or trimetric system, or of either of the oblique. The latter is accomplished by causing the hoop to revolve, and for the triclinic the vertical hoops come into action. Adjustment of the weights leads to an alteration in the axes, and the relations of the weights for a special form may be studied.

It is evident that the dodecahedron and trapezohedron may be produced in this instrument as in the first, and that the forms due to truncating or bevelling of the sides are obtained very readily.

The following are the advantages of the apparatus:—First, it shows clearly the effect of changes of force in producing changes of form. The weights can be approximated or separated, and thus the relations of allied forms may be studied. The number of weights may be increased, and the change of form by grouping may in this way be well shown.

If we take an india-rubber tissue ball inflated with air as an example of an infinite number of forces acting from a centre, and a piece of stretched cord with a weight attached as the other extreme limit, many of the intermediate conditions where strings are made to form the edges of figures may be easily understood from the arrangements I have described.

It is true that such methods as are here suggested are open to the objection that mathematical principles of a very important kind are involved. I think the same objection may be made to any mechanical contrivance; but so far from getting rid of a difficulty without explaining it, I hold that the apparatus, whilst it will produce a better conception of crystal forms, and the actual work in the crystals themselves, in the minds of those students who know very little about mathematics, viz., almost all students of chemistry and mineralogy, and a still larger number of geological students, the apparatus will prove useful to mathematical students inasmuch as the arithmetical processes are tedious and complex for even those forms in which the mathematical relations are comparatively simple. For the forms with oblique axes the advantages of a simple method of noting the weights necessary to maintain equilibrium far outweigh the disadvantages.

Note.—Professor Wiltshire informs me that many years ago, Mr. Mitchell, at the Royal Institution, showed a model by which the derivation of the crystalline systems from the octahedron was explained.

EXPLANATION OF PLATE III.

Fig. 1.—α', β', γ', δ', ϵ', η', octahedron.
p, p', weights.
α, β, γ, δ, ϵ, ζ, η, θ, double hexagonal pyramid.
Fig. 2.—a, b, c, d, e, f, octahedron.
a', b', c', d', e', f', g', h', cube.
A, B, C, D, E, F, weights.
Fig. 3.—The upper figure shows the hexagonal prism surmounted by hexagonal pyramids. The lower shows the rings with the rhombohedron formed.

XXV. *A Shunt-Transformer.* *By* Mr. E. W. SMITH*.

ALTHOUGH this experiment has already been described by Professors Ayrton and Perry in a paper at the Institution of Electrical Engineers, it was thought to be worth while occupying this Society's time in showing it here, as it forms a good lecture-experiment, if nothing more, to illustrate acceleration and lag of alternating currents.

The experiment consists as follows:—Between two leads a certain alternating potential difference, V, is maintained. We have two resistances, A and B (fig. 1), in series, through which part of the current flows. If the impedance of A is equal to that of B, then P.D. at terminals of A, V_a, and P.D.

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* Read June 8, 1889.

at terminals of B, V_b, are equal. If A and B are alike in resistance and self-induction, then V_a and V_b would be in the same phase and each equal to $\frac{1}{2}$V. If curve 0 0 0 0 (fig. 2) represents V, then curve 1 1 1 1 represents $\frac{1}{2}$V. But if A has large resistance and B much self-induction, then, although V_a may be still equal to V_b, V_a will be lagged and V_b accelerated. Then curve 3 3 3 3 will represent V_a, and 4 4 4 4 V_b.

Fig. 1.

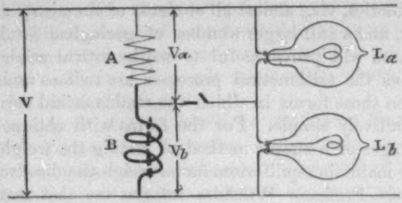
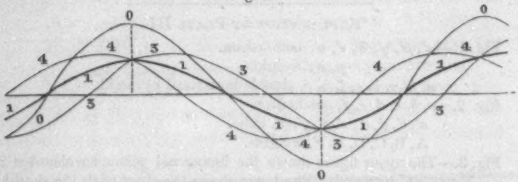


Fig. 2.



We have also two similar sets of lamps, L_a and L_b (fig. 1) in series between the two main leads. Under ordinary circumstances they would each have a P.D. of $\frac{1}{2}$V (curve 1 1 1 1, fig. 2) at their terminals. But if we connect the junctions of the two sets of lamps with the junction of the two resistances, then lamp L_a will have P.D. V_a, and lamp L_b a P.D. V_b; and therefore *both lamps will become brighter*. If the lamps take an appreciable current, then, when the junctions of the different circuits are joined, the current in the inductive resistance as well as in the lamps becomes greater, but that in the *main circuit becomes smaller*, as may be seen by a dynamometer.

The resistance A may of course consist of lamps, and B may be a choking coil, which absorbs very little energy.

Prof. Ayrton has given this inductive part of the circuit the name of a "Shunt-Transformer."

I have made a similar experiment with one of Mr. Mordey's transformers wound with three coils, each having the same number of turns. Using one of these as a primary and the other two as two independent secondaries, then, by having one circuit comparatively non-inductive and the other inductive, one gets the arithmetic sum of the amplitudes of the secondary currents *greater than that of the primary current*, although of course the vector sum must be less. For instance, take one particular experiment. The primary was on a circuit having a P.D. of 128 volts. One secondary, A, was composed of lamps, and the other, B, was a Tesla motor.

Volts in primary,	128	Current in primary,	14·2	
„ secondary A,	119	„ secondary A,	8·0	16·2
„ secondary B,	119·5	„ secondary B,	8·2	

Connecting up the secondaries of transformers A and B in parallel, the volts at the terminals of the primary being the same as before, and the secondary circuit being lamps.

Volts in primary,	128	Current in primary,	17·4
„ secondary,	121	„ secondary,	16·3

Here we have to give 17·4 amperes to the primary instead of 14·2 to get same current in secondary, and the volts in secondary are practically the same as before.

If we were being supplied with electricity, what should the meter measure? Surely the amount of energy we use. But ordinary meters only measure ampere-hours, and so cannot but give records in favour of consumer or supplier. The sooner the public understand this, the sooner we shall have a scientific meter in our houses.

XXVI. *On the Use of the Biquartz in determining the position of the Plane of Polarization.* By A. W. Ward*.

The biquartz has been so often used, especially on the Continent, by investigators on the rotation of the plane of polarization of light, and with such extremely varying degrees of accuracy, that it seems of interest to account for these results mathematically. Verdet and H. Becquerel obtained results which varied by less than 4′; while Wertheim, Matteucci, Edlund, Lüdtge, and Villari obtained results varying by as much as 2°. Lüdtge has in one case obtained a rotation of 4° where, on his own showing, the light was circularly polarized. Verdet's and Becquerel's accurate results were obtained when rotation was looked for in liquids and isotropic substances; and the inaccurate results of Wertheim &c. were obtained when seeking for a rotation in doubly-refracting substances. In the former case the light remained plane-polarized, in the latter it became elliptically-polarized, and the position of the plane of polarization was really that of one of the axes of the ellipse. In the present investigation we shall then determine with what degree of accuracy the biquartz can be used to determine the position of the axes in elliptically-polarized light.

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* Read June 8, 1889.

Let us suppose that the elements of the elliptically-polarized light are given by the displacements along the axes of the ellipse, and by the inclination of an axis of the ellipse to some direction fixed in space. Let the displacements ξ and η be parallel to the axes of the ellipse, and let the axes of x and y be fixed in space, z being the axis along which the light travels; and let ω be the angle between the axes of x and ξ. If then c^2 be the intensity of the light, $\tan\gamma$ the ratio of the axes of the ellipse, the vibrations of the light are given by the equations

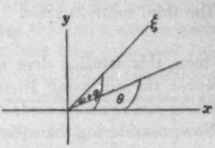
$$\left.\begin{aligned}\xi&=c\cos\gamma\cos\frac{2\pi}{\lambda}(vt-z),\\ \eta&=c\sin\gamma\sin\frac{2\pi}{\lambda}(vt-z).\end{aligned}\right\}\quad .\quad .\quad .\quad .\quad (1)$$

The angles γ and ω are known whenever we know the history of the light; how it became converted from plane-polarized into elliptically-polarized. If, for instance, the change took place in passing through a doubly refractive medium whose axes are those of x and y, then

$$\left.\begin{aligned}\tan 2\omega&=\tan 2\alpha\cos\beta,\\ \sin 2\gamma&=\sin 2\alpha\sin\beta,\end{aligned}\right\}\quad .\quad .\quad .\quad .\quad (2)$$

where β is the total angular retardation, and α the inclination of the initial plane of vibrations to that of xz. In these equations β is a function of λ, viz. $\frac{2\pi}{\lambda}(\mu_1-\mu_2)z$, where μ_1 and μ_2 are the indices of refraction along the axes of x and y respectively. If α is small, variations in ω due to λ are not important; but if α is large this is no longer the case, as we shall eventually see.

Let us now pass the light (1) through a biquartz which is such that the plane of polarization of light, of wave-length λ, is turned through an angle ϕ. This rotation will simply turn the ellipse as a whole, and not affect the ratio of the axes. Hence for upper half of the biquartz ω becomes $\omega+\phi$, and for lower half $\omega-\phi$.



Let the light be now analysed by a Nicol whose plane of vibrations makes an angle θ with the plane of xz. If then h^2 be intensity of light passing through the upper half of biquartz, and k^2 that of light passing through the lower half, we have, as usual,

$$\left.\begin{aligned} h^2&=c^2\cos^2\gamma\cos^2\overline{\omega+\phi-\theta}+c^2\sin^2\gamma\sin^2\overline{\omega+\phi-\theta}, \\ k^2&=c^2\cos^2\gamma\cos^2\overline{\omega-\phi-\theta}+c^2\sin^2\gamma\sin^2\overline{\omega-\phi-\theta}; \end{aligned}\right\} \quad (3)$$

or

$$\left.\begin{aligned} h^2&=\frac{c^2}{2}\{1+\cos 2\gamma\cos 2(\omega+\phi-\theta)\}, \\ k^2&=\frac{c^2}{2}\{1+\cos 2\gamma\cos 2(\omega-\phi-\theta)\}. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (4)$$

We have now to determine what value of θ makes $h^2=k^2$ *for all values of* λ.

Equating h^2 to k^2, we get

$$\cos 2\gamma\sin 2\phi\sin 2\overline{\omega-\theta}=0. \quad . \quad . \quad . \quad . \quad . \quad (5)$$

This equation is satisfied whenever

$$\cos 2\gamma=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

or

$$\sin 2\phi=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

or

$$\sin 2(\omega-\theta)=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

The first of these solutions occurs when $\gamma=\frac{\pi}{4}$, *i. e.* when the elliptically-polarized light is really circularly polarized. In this case the phrase plane of polarization has no meaning at all, and so it need no be discussed.

The second solution (7) gives $\phi=\frac{\pi}{2}$. This can only be the case for one particular wave-length, and depends simply on the thickness of the biquartz. A biquartz is usually made of such a thickness that ϕ is $\frac{\pi}{2}$ for the yellow light from the brightest part of the spectrum. We shall suppose this to be the case here.

The third solution gives

$$\omega=\theta.$$

If, then, this solution does not hold for all values of λ, then, however the analysing Nicol be turned, both halves of the biquartz can never be made of the same uniform tint.

Now, considering the equation

$$\tan 2\omega=\tan 2\alpha \cos\beta,$$

we see that $\omega=\alpha$ always if $\beta=0$, that is, if the incident plane-polarized light always remains so. If β is not equal to 0, then still $\omega=\alpha$ for all values of λ, if $\alpha=0$ or $\frac{\pi}{4}$. If α is 0, then $\gamma=0$, and the light is plane-polarized as before. The case we have to discuss, then, is $\alpha=\frac{\pi}{4}$.

If $\alpha=\frac{\pi}{4}$, and also for any particular value of λ, $\beta=\frac{\pi}{4}$, then light of that colour is circularly-polarized. Hence, however we alter θ, no change in the intensity of that light will take place. If this circularly-polarized light comes from a prominent part of the spectrum, it will be impossible to note small change in the tint of passage due to the varying presence of other colours. The difficulty experienced will be precisely similar to that of fixing the position of the plane of polarization by means of the yellow tint of passage instead of the violet tint.

If β is never so great as $\frac{\pi}{2}$, then, when both halves of the biquartz are of the same uniform tint, the position of the analyser determines the position of the ellipse; but the uniform tint will not be that due to excluding the yellow light of the spectrum, but will contain lights of every colour, but not in that proportion which constitutes white light. The tint may be rosy or yellow.

If α is neither 0 nor $\frac{\pi}{4}$, then ω varies with λ, and cannot possibly satisfy the solution for all values of θ. In this case, then, both halves of the biquartz can never be made of the same tint. As this is the general case, we conclude that the biquartz is not a suitable instrument to use when, instead of plane-polarized light, we have elliptically-polarized.

The following table gives the values of ω due to variations in α and λ when the light has passed through a quarter undulation-plate of quartz. The values have been calculated from Rudberg's table of indices, quoted on p. 317 of Glazebrook's 'Optics.' The capital letters refer to the lines of the spectrum.

		C.	D.	E.	G.
	β.	80°.	90°.	100°.	126°.
$\alpha=10°$.	ω	2°	0	−2°	−6°
$\alpha=20°$.	ω	4°	0	−4°	−13°
$\alpha=44°$.	ω	39°	0	−39°	−43°

The above is simply given as an illustration of the magnitude of the quantities involved in a particular case where it is easy to make the calculations. I have tried the experiment by passing light the reverse way through an elliptic analyser (*i. e.* a Nicol prism and quarter undulation-plate), then through the biquartz, and finally through an analyser. It is found quite impossible to get any match between the two halves of the biquartz when α is large. The actual dispersion of the axes depends upon the variations of β with λ, and this is very much greater in quartz than in such a doubly refracting substance as compressed glass. But in most cases there will always be sufficient variation to make the use of the biquartz a very unsuitable method, and this does, I think, account for the two classes of results mentioned at the beginning of this paper.

In conclusion, I have only to express my gratitude to Mr. Glazebrook for many valuable suggestions, and to Professor J. J. Thomson for the use of the Cavendish Laboratory.

XXVII. *Intermittent Lightning-Flashes.* By H. H. Hoffert, *D.Sc., A.R.S.M., Demonstrator of Physics at the Normal School of Science and Royal School of Mines*[*].

[Plate IV.]

The storm which passed over London on the evening of June 6th afforded an unusually favourable opportunity for observations, both with and without the aid of the camera, on the character of lightning-flashes, and for determining the causes of some of the curious effects noticed by previous observers.

While watching the storm from my house in Ealing I could in several instances distinctly perceive a flickering appearance in a discharge, and in one particular case the repetitions were at least 5 or 6 in number, just sufficiently slow for the eye to detect the variations in brightness without removing the impression of one single flash. Other observers with whom I have since spoken have informed me that they had observed a similar effect, and that in some instances flashes, following as nearly as could be seen in the same path, were separated by an appreciable interval, often of several seconds' duration. Photographs of lightning have frequently been obtained showing banded, ribbon, or double flashes; but, so far as I am aware, these have not been of so decided a nature as to remove doubts whether the appearances could not be ascribed to the effects of halation by reflexion from the back of the negative, or to blurring from the photograph having been taken through the glass of a window, or to some other similar cause. I was therefore anxious to obtain some decisive evidence of the repetition of the flashes, and as my friend Mr. G. J. Snelus was at my suggestion attempting to obtain some photographs of the lightning, I joined him, and he kindly placed his camera and some plates at my disposal.

The rain having ceased for the moment, I was able to go out on to a balcony and thus get a good view of the storm, which had now approached quite close and seemed to surround us. The camera was held in the hand and pointed towards

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[*] Read June 8, 1889.

INTERMITTENT LIGHTNING FLASHES.

Fig. 1.

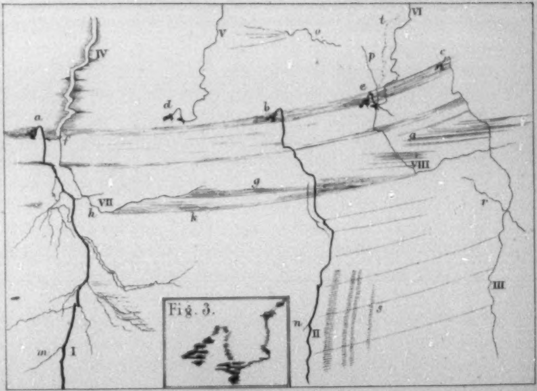

Fig. 2.

Mintern Bros. lith.

the north-east, where, about half a mile away, numerous brilliant flashes were occurring. The cap of the camera was taken off, and then the camera was moved in a horizontal plane about the lens as a centre at the rate of about once to and fro in three quarters of a second, until a flash was observed in the direction in which it was pointed, when the cap was at once replaced. The duration of the exposure of the plate was about half a minute. The camera was of quarter-plate size, the plates were Ilford rapid, and the lens, which was a rapid rectilinear, was used with full aperture (*f*/8).

I hoped, by having the camera moving, to be able to separate the successive components of the flashes, and in this I was fortunately successful. In fig. 1 (Pl. IV.) is a reproduction of one of the photographs obtained, and on it can be seen two triple flashes (I., II., III., and IV., V., VI.), and one double flash (VII. and VIII.).

During the interval that the plate was exposed the illumination of the sky from flashes out of the line of view, or behind clouds, produced the glare seen in the centre and upper part of the photograph, and some faint flashes which were noticed were probably the cause of the streaks seen in fig. 2 at *o*, *p*, and *r*.

A careful examination of the photograph reveals many interesting features. The three successive flashes I., II., III. are identical in form. If the negative be placed over a print so that either of the three lines on the negative lies over either of those on the print, the coincidence is seen to be exact even to the smallest irregularities. Nevertheless, of all the branch-flashes which spread out from I. only a small trace exists at *n* in II., and none whatever in III. Sweeping across the photograph and connecting corresponding points in the successive flashes are streaks of light, showing that a very considerable residual illumination remains between the discharges. These streaks are especially well marked between the components of the double flash VII., VIII., and are also very bright along the path of the head of flash I. They are not always present; for in another photograph obtained upon a moving plate by Mr. Snelus, in which a flash is reduplicated, there is no trace of them, the flashes being quite sharp and distinct. The streaks commence abruptly with one discharge and end abruptly with another. Their extension to the left of I. was probably due to the camera not having quite reached the end of its swing. Where they are brightest there is in all cases a swelling at the part of the flash where they start.

At the upper left-hand corner there is a curious dark flash with bright edges and short luminous streaks. The form of this dark flash is exactly reproduced in the two bright flashes V. and VI. Dark flashes are frequently met with in photographs of lightning, and have been usually ascribed to reversal of the image by overexposure; but I do not think this explanation applies in the present case, both on account of the appearance which the dark flash presents, and also because as far as I can recollect the brilliant downward flash I. appeared to the eye much the brightest of those that occurred while the plate was exposed. There is a similar but less distinct dark gap to the left of VI.; and to the right of II. are three or four faint bands parallel to it and following its sinuosities. These all seem to be due to variations in brightness in the luminous streaks, which are thus shown to be electrical and not phosphorescent in character.

When carefully compared, the forms and positions of the flashes I. to VIII. are found to present such agreement as to show that they must have formed a system of discharges closely connected together. If V. be placed over IV. so as to coincide with its left-hand border, the portion *d* exactly coincides in form and position with the portion *a* of I., and both *a* and *d* terminate in a curious bifurcated enlargement which, when examined with a lens, shows a beaded appearance, roughly sketched in fig. 3. This coincidence of form and position would be difficult to account for if I. and IV. were independent flashes not occurring at the same time. It is evident, however, that the series I., II., III. cannot have occurred during the same sweep of the plate as IV., V., VI., since the curve joining *a*, *b*, *c* is concave upwards, while that joining *a*, *d*, *e* is concave downwards. The flashes VII., VIII. seem also to have formed part of the same system of discharges, for the part of VII. from *f* to just above *h* is coincident with the upper portion of I., while the luminous streaks extending between VII. and VIII. agree in their directions with the curves joining *a*, *b*, *c* and *a*, *d*, *e*, those at *g* being parallel to the line joining *d* and *e*, and those at *k* following nearly but not exactly the line of *a*, *b*, *c*. There is thus afforded some clue to the determination of the order of the discharges, and I think the order was probably as follows:—VII., VIII., VI., V., IV., I., II., III., the first two occurring during one sweep of the camera; VI., V., IV. during the backward sweep; and I., II., III. in the next onward sweep. There must thus have been an interval of a little over a second between the first and last discharge, for the motion of the camera was at about the rate of three quarters of a second for a complete swing to and fro. The interval between the successive discharges was, therefore, between the fifth and tenth of a second. Had it been much longer the flash would have presented to the eye a flickering appearance.

Whatever be the explanation of some of the effects noticed above, it is evident that a lightning flash has not the simple instantaneous character formerly supposed; but that it consists of a varying number of successive discharges following one another in the same path at intervals which may in some cases be comparatively long.

Note.—Since the above was written I have had an opportunity of inspecting the photographs of banded, ribbon, and curtain lightning collected by the Royal Meteorological Society. There is, I think, no doubt that the explanation of these is afforded by the multiple flashes and luminous streaks noticed above. It is noteworthy that they were always obtained with a camera held either in the hand or in such a way as to render motion probable.

Prof. Herschel, who had taken photographs during the same storm, referred to the fluttering appearance of the flashes and on their long duration; in many cases the time was sufficient to allow him to direct the camera towards the flash and make a successful exposure. He had also observed multiple flashes with the unaided eye, and on waving his hand about he had sometimes noticed about a dozen distinct images of it during one discharge.

Mr. Gregory said that he watched the storm along with two others, and they could seldom agree as to the shape of the flashes or on their simple or multiple character. The want of agreement as to multiplicity he thought might be caused by their eyes being directed towards different parts of the sky when a multiple flash occurred ; the one who happened to be looking towards the flash might be conscious of only one impression, whereas the others in directing their eyes would receive the flashes on different parts of the retina. In some cases as many as three distinct flashes (occurring at intervals of about ten seconds) traversed the same path, and a number of the discharges presented a beaded or striated appearance. The beads seemed to remain after the main flash had faded, and this might account for the bands shown in Dr. Hoffert's photograph.

Mr. C. V. Boys, in referring to multiple flashes, said that although his statements made in the discussion of Mr. Whipple's paper on April 13th were not readily accepted, yet no one who watched the recent storm could doubt their existence.

Prof. S. P. Thompson thought the order of the flashes on the photograph may have been the reverse of that supposed ; for he observed that the bands of light extended on both sides of the (so-called) primary flash, whereas the outside of the third flash was quite dark.

Mr. E. W. Smith noticed many cases of "sympathetic discharge" in which a flash in the north seemed to precipitate another in the north-west within a few seconds ; and in this he was corroborated by Mr. Gregory, who viewed the storm from a different locality.

Mr. C. V. Burton thought the heating of the air by the first spark of a multiple flash might give rise to the tributaries to the succeeding sparks.

Mr. A. W. Ward mentioned a long flash observed at Cambridge which passed from the zenith and struck some farm-buildings at a distance, and he was particularly impressed by the considerable time occupied in its progress.

XXVIII. *Note on some Photographs of Lightning and of "Black" Electric Sparks.* *By* A. W. CLAYDEN*.

DURING the thunderstorm on the night of June 6 I exposed several plates in the hope of securing photographs of lightning. Three of these gave results.

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* Read June 22, 1889.

One was exposed to two flashes, not counting such as did not cross the field of view. These two flashes show complicated and beautiful structure. One of them is a multiple flash, distinctly seen as double by both my wife and myself. An enlargement of this shows curious flame-like appendages pointing upwards from every angle. The other flash is a broad ribbon. The images of the masonry in the left-hand corner (which are necessarily slightly out of focus) show three positions of the camera. They are sharp, hence the camera did not move during the existence of a flash; and the directions of those movements which did occur do not in any way correspond to the movements (if such there were) which would have been required to produce the ribbon-like effect from a linear flash.

A second plate shows four flashes, and the camera moved much more than in the first case. None of these flashes are ribbons. Development showed the plate to be overexposed.

The third was exposed to six flashes; that is to say, I judged that six of them crossed the field of view. There were many others between times, which were either in the clouds or occurred in other parts of the sky. One flash, I remarked at the time, must be "right down the middle of the plate." Development showed this plate to be very much overexposed, and the image required careful nursing. I was much surprised to see nothing but one triple flash in the corner. I supposed that I must have mistaken the plate, and was about to throw it away, but on carefully searching for the above-mentioned vertical flash, I found its image was reversed, printing as a black flash with a white core. Subsequent observation showed other dark flashes; and the enlargement of part of the plate shows that there are indications of white cores to each of them.

Now the connexion between this reversal and overexposure was very striking. Hence it occurred to me that the black flashes might be due to a sort of cumulative action, not to the excessive brightness of the individual flashes, but rather to the excessive action produced by the superposition of the glare from an illuminated white cloud upon the normal image of the flash.

To test this I endeavoured to obtain the same effect with the sparks from a small Wimshurst machine; but, under the conditions in which I worked, I could not get a longer spark than one inch.

I first photographed a series of brilliant sparks, using two large Leyden jars. These gave normal images, very dense, and shaded off at the margins, although the focus, as shown by the knobs of the machine, was good.

Next I tried less brilliant sparks from the machine with its ordinary small jars. These gave similar images, but less dense.

Then I repeated both experiments, and before developing the plates exposed them to the diffused light from a gas-flame. The brilliant sparks then yielded images which may either be called normal with a reversed margin, or reversed with a normal core. The fainter sparks were completely reversed.

One plate of bright sparks was exposed to the gas-light, so that different parts were acted upon for different times. The reversal seems to spread inwards as the exposure to diffused light is increased.

One plate of faint sparks was only half of it exposed to diffused light. The result is that on that part the sparks are reversed, while on the other they are normal.

Finally I photographed a number of sparks in a series across the plate, and placed a sheet of white cardboard behind them to do duty for the white background of cloud. Some of the first sparks impressed on the plate show reversed images.

Coupling these experiments with the observations as to the overexposure of the "dark-flash" plate, and with the fact that all dark-flash plates I have seen show symptoms of considerable exposure, I submit that there is at least a good case for this theory of cumulative or repeated action producing the reversal. The partial reversal of the bright sparks seems to correspond with the bright core to some dark flashes; and the complete reversal of the less brilliant sparks to the absence of any such core from the less conspicuous portions of a dark flash.

There is certainly one difficulty yet to be got over, and that is the crossing of a dark flash by a bright one. However, I have some experiments* in view which I hope may throw some light upon this also. In my own negative the point of crossing seems to be extra bright.

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* Since writing the above communication I have made a number of further experiments, which I hope to describe in detail at some future

Meanwhile I must apologize to the Society for bringing forward these notes in such an immature and hastily constructed condition. My excuse must be that the photographs of electric sparks were only taken the day before yesterday, and today's meeting is the last of the session.

In the discussion which followed, Mr. W. N. Shaw exhibited a photograph taken during the same storm, which is particularly rich in dark flashes branching outwards from an intensely bright one. In some places the bright line has dark edges, and in one part a thin bright line runs along the middle of an otherwise dark portion of the flash. In answer to Mr. Inwards, Mr. Shaw said the plate was exposed about half a minute, and the former thought that under those conditions the appearance of the plate did not contradict Mr. Clayden's hypothesis.

Speaking of the same photograph, Prof. Perry considered that Mr. Clayden's observations would explain the result, for a bright flash required more exposure to diffused light to reverse it than a faint one did.

Prof. Ramsay reminded the meeting that Prof. Stokes's "oxides of nitrogen" explanation was still a possible one; and Mr. C. V. Burton asked whether they may be due to faint sparks cutting off light from brightly illuminated clouds, just as a gas-flame absorbs light from a brighter source.

In reply, Mr. Clayden thought the "oxides of nitrogen" hypothesis improbable, and said his experiments did not enable him to answer Mr. Burton's question. As regards Mr. Shaw's plate, he believed the diffused light from the clouds would be

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time. But perhaps I may be allowed to say at once that I have succeeded in imitating the phenomenon of a bright image crossing a dark one. The experiments point to the conclusion that diffused light acting upon a plate can reverse previously impressed images of electric sparks, but is powerless to affect any such impressions which may be made afterwards. Similar results are obtained whether the source of the diffused light is a gas-flame, a lamp, or a series of sparks. I do not at present offer any theoretical explanation of these facts, but they are in themselves sufficient from a meteorological point of view. "Dark" flashes of lightning have no existence in nature, but are caused by the exposure of the plate to an illuminated sky *after* the passage of the flash. This illumination may be due to subsequent flashes, the more recent of which will give normal images possibly crossing the reversed ones.

sufficient to reverse the fainter tributary flashes, although it was insufficient to reverse the primary one.

From data obtained when the ribbon-flash was taken he had made some calculations, which gave the height of the clouds about 1000 yards and the ribbon-flash 1300 yards long and 100 yards wide.

XXIX. *Expansion with Rise of Temperature of Wires under Pulling Stress.* *By* J. T. BOTTOMLEY, *M.A.*, *F.R.S.*, *F.C.S.*[*]

[Plate V.]

IT is probably well known to the members of the Physical Society that, at the instance of the British Association and with the assistance of a money grant from that body, very interesting secular experiments on the elasticity and ductility of wires were commenced some years ago in Glasgow. In the tower of the Glasgow University buildings certain wires are hung in pairs for comparison. One of each pair carries a heavy load about half the breaking weight of the wire; the other carries about one tenth of the breaking weight. Certain marks are put on the wires; and the object of the experiment is to find whether the heavily loaded wire seems, on comparison with the lightly loaded wire, to go on running down incessantly, or whether it comes asymptotically to a fixed length for a given temperature, ceasing to experience further permanent elongation.

The observations of the last few years show that the elongation due to further pulling out has, to say the least, become exceedingly small, so small that it is extremely difficult to observe it; and at the Aberdeen meeting of the British Association I pointed out that a great difficulty is introduced into the making of deductions from these observations through the impossibility of controlling the temperature of the tube in which the wires are placed. If, for example, there is any difference as to expansion with temperature of the same wire when lightly and when heavily loaded, a cause of disturbance would be introduced which it would be excessively difficult to allow for. It seemed therefore absolutely essential to make direct experiments on this point. The

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[*] Read June 22. 1889.

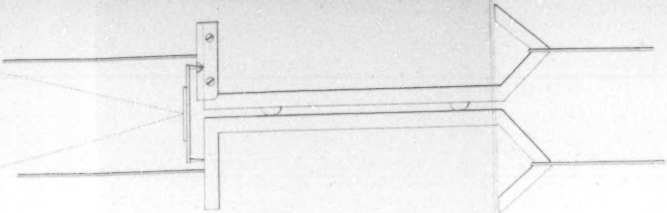

Fig. 3.

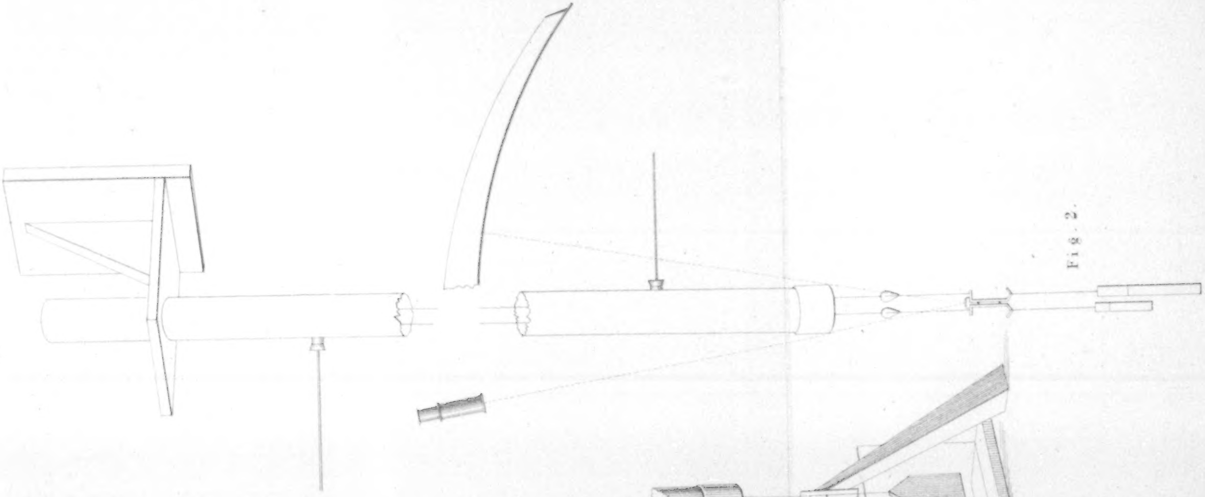

Fig. 2.

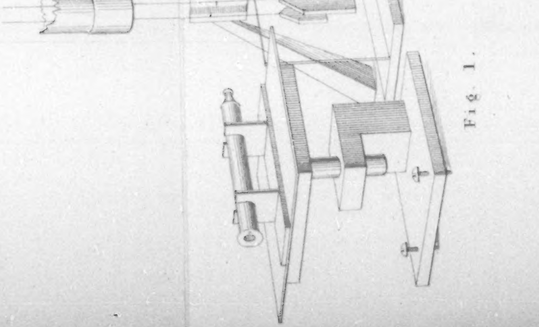

Fig. 1.

Mintern Bros. lith.

object of the present communication is to give an account of some experiments of this kind. A preliminary account of these experiments was communicated to the British Association at the Manchester meeting (1887), and was printed in the Philosophical Magazine for October of that year.

The wires hung up in the tower of the Glasgow University building are two of platinum, two of gold, and two of palladium, these wires being chosen on account of their small liability to oxidation. The wires on which I have experimented up to the present time have, however, been of copper and platinoid. The latter metal is an alloy* of nearly the same composition as German silver, but containing a small quantity of tungsten and made in a peculiar way.

The figures show the arrangements for experimenting. A long tube of tin-plate about 2½ inches in diameter was set up vertically, fixed by means of brackets at two or three places. This tube has inlets and outlets for steam, of which I have a plentiful supply in the laboratory from boilers connected with the University apparatus for heating and ventilation. It has also openings for thermometers. The length of the tube was 17½ feet in the experiment with copper wire, and somewhat shorter in the platinoid experiment.

A piece of excellent copper wire was taken, and its breaking weight was found to be 750 grammes. Its diameter was 0·22 millim. Two portions of this wire were hung side by side in the centre of the tube. In order to suspend them their ends were passed into two small trumpeted holes in a stout brass plate and soldered to the back of the plate. The plate was screwed up to a strong beam in the ceiling of the laboratory. This forms by far the best mode of supporting a wire for experiments on elasticity. One of the wires carried 75 grammes, the other 375 grammes.

A few preliminary experiments as to heating and cooling revealed a difficulty the magnitude of which I was unprepared for. When the steam was admitted into the tube the wires of course expanded, the heavily loaded wire going down far more than the other; and when the steam had been stopped and the tube allowed to cool, they contracted again but not to the same extent; and neither came back to its original length. This was of course to be expected. But it turned out, on repeating the heating and cooling, that the same thing occurred again and again; and it was not till after about 150 heatings and coolings that the heavily loaded wire assumed a permanent state, expanding and contracting by equal amounts with the heating and cooling*. The lightly loaded wire took its permanent condition much sooner. This itself was a valuable result, applying directly to the early observations on the secular wires in the University tower.

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* Invented and patented by Mr. F. W. Martino of Sheffield.

Fig. 1, Pl. V., shows the arrangement for these preliminary experiments. Behind the wire a half-millimetre scale was put up; and each wire carried a pointer moving over the scale. The readings at hot and cold temperatures were taken with the well-known Quincke microscope-kathetometer; and the process and observations were carried on, as has been said, till each pointer gave unvarying readings at the hot and cold temperatures. It was then considered that the wires had assumed a permanent condition.

The pointers and scale were now removed and two hooks, of peculiar construction, figs. 2 and 3, were attached to the ends of the wire, the wires being passed into holes made for the purpose and soldered in. These hooks carried and formed part of the stretching weights. The upper parts of the hooks are turned over to form two horizontal plates, and the vertical parts of the hooks press very lightly against each other and form almost frictionless guides one for the other. In one of the vertical faces a vertical V-groove is cut, while the remainder of the face is plane and well-polished. Two little feet on the vertical face of the other hook move in the V-groove of the first, and a third foot rests against the smooth vertical face. A relative geometrical guide is thus provided for the hooks, and the shape of the hooks is such that the gravity of the whole, including the weights, gives the requisite slight pressure of the one against the other. The horizontal parts of the hooks just mentioned carry what is practically a small three-legged table, of which two legs rest on one platform and the third on the other. To be more precise, one of the platforms carries on its top a little plate with a V-groove cut in it; and a knife-edge, cut away at the central parts and thus leaving two feet at the extremities, attached to the table, rests in the V. A third foot, rounded, rests on the other platform, which is plane and polished. On the top of this little table, which is a square of about $1\frac{1}{2}$ centimetre in the side, there is fastened a perfectly plane parallel Steinheil mirror; and a telescope with cross wires, looking down very nearly vertically on the mirror, views, reflected in the mirror, a half-millimetre scale suitably placed.

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* I must not fail to express here my indebtedness to Mr. Thomas A. B. Carver, assistant, and Mr. W. S. Cook, student in the Physical Laboratory, who carried out these experiments in the winter sessions 1887–8 and 1888–9 respectively. Without their patient labour the work would have been impossible to me.

It will be seen at once that if the two wires were to elongate equally with rise of temperature, their extremities would go down together and almost the only effect (not absolutely of course) on the scale-reading would be to alter somewhat the focus. But if one wire elongates more than the other the mirror is tilted, and the change in the scale-reading readily gives the amount of relative displacement of the ends of the wire.

The arrangement works in the most satisfactory way, and it now only remains for me to state the results. I must remark, however, that it was exceedingly difficult to make an exact estimate of the temperature of the tube, even when the steam was running strongly through it. Thermometers inserted by means of corks in holes provided for the purpose showed that differences of 2° or 3° (I think not so much as 5°, however) existed at different parts of the tube.

This being understood, I may say that the range of temperature in the various experiments was from 15° C. or 16° C. (cold) to 98° or 99° (hot), or about 83° C. The length of copper wire experimented on was 530 centimetres. The difference of expansions observed was 0·14 millim. or 0·014 centimetre, the heavily loaded wire going down most. This gives a relative expansion of 26×10^{-6} per centimetre for a change of temperature of 83°; or $0·314 \times 10^{-6}$ per centimetre per degree.

I find the linear expansion of copper per degree stated at about $17·2 \times 10^{-6}$, and thus the ratio of this extra expansion to the total expansion is 3·14/172, or about $\frac{1}{55}$.

With regard to platinoid wire—after more than three months of daily heating and cooling, the wires (0·35 millim. in diameter) came to a thoroughly permanent condition. On a length of 490 centimetres a relative extra extension of 0·111 millim. or 0·011 centim. was observed for a change of temperature of 83°, and as with copper the heavily loaded wire experienced most elongation. These numbers give $22{\cdot}4 \times 10^{-6}$ as the extra expansion per centimetre, or $0{\cdot}27 \times 10^{-6}$ per centimetre per degree Centigrade.

The linear expansion of platinoid was unknown, though it might be supposed to be something not very different from that of German silver. Accordingly a series of experiments were carried out on this question, with the result that the linear expansion of the specimen used was found to be ·0000154 per degree Centigrade.

The relative extra expansion of platinoid wire is therefore $\frac{2{\cdot}7}{154}$, or $\frac{1}{57}$.

Mr. H. Tomlinson thought the probationary period for copper might be considerably shortened by repeatedly putting on and taking off the load, and by subjecting the wire to torsional oscillation. With iron wires this would not be the case, for they behave in a most peculiar manner, and require long periods of rest after each oscillation. From experiments he had conducted during the last two years, he found that the permeability of iron could be enormously reduced by repeated heatings and coolings whilst undergoing magnetic cycles of small range.

Mr. Gregory said the paper threw considerable light on some experiments on the lag of stretched wires upon which he was engaged. He also suggested heating the wires by electric currents.

In reply, Mr. Bottomley said he had considered it important to leave the wires untouched after being suspended; and, as regards heating by electricity, he thought that convection-currents would make the temperature non-uniform.

XXX. *On a Relation existing between the Density and Refraction of Gaseous Elements and also of some of their Compounds. By* Rev. T. Pelham Dale, *M.A.*[*]

In my former paper which I presented to the Society[†] I touched upon the empirical relations discoverable between the specific refractive energies of selenium and sulphur. The present state of the data, however, relating to these substances is so far unsatisfactory that we are not furnished with refractive indices and density taken from the same specimen at the same temperature. Thus there is a doubt as to the density to be chosen as normal, in consequence of the difference due to the allotropic conditions in which these substances are found. This difficulty may be to a certain extent eluded by taking both densities and calculating the specific refractive energy for both. The result is, in the case of selenium and sulphur, that in both substances the mean values are not very far apart.

The question, however, of the relation between the refractive indices of the elements is of so great interest, that it appeared advisable to calculate the value of $\frac{\mu-1}{d}$ of all chemical elements for which data existed, giving both refractive index and density in the state of gas or vapour. This would include the refractive indices of hydrogen, oxygen, and nitrogen, and the importance of these is obvious. Then, again, the calculated densities derived from atomic weights might be used in cases where an observation was wanting, and as a check where these existed. A very few trials revealed relations which it was impossible to overlook.

These relations among the numbers found are set forth in the accompanying Tables.

These tables are arranged in columns. The first, column I., contains the name of the substance. Column II. its index of refraction less unity, or $\mu-1$. Column III. its density. Column IV. its specific refractive energy, or $\frac{\mu-1}{d}$. Column V. the ratio of $\mu-1$ in the substance to the similar number for hydrogen. The upper row of numbers in each line are natural numbers, and those immediately under are the mantissæ of their common logarithms. Examining this table in detail, it is seen at once that the logarithms of the specific refractive energies of nitrogen, chlorine, and phosphorus are nearly identical, as also of sulphur and oxygen. It will also be observed that in N, Cl, P this log. is double of that of H, and S and O three times that of half the log. specific refractive energy of hydrogen. In the same way, the log. of the specific refractive energy of mercury is 8 times, and arsenic 9 times this quantity.

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[*] Read May 25, 1889.

[†] *Ante*, p. 17.

If we turn to column V. we find that oxygen has nearly double the refractive energy of hydrogen, mercury 4, arsenic 8, sulphur, but not so closely, 12 times.

Now all these coincidences arise from observation only, and are independent of theory altogether. The probability that they should be fortuitous is very small in so large a number of instances.

When we turn to the compounds, we again see indications of the same law. Thus N_2O has log. specific refractive energy half of similar log. of CO ; and the refractive energy of Cy is very nearly 6 times that of hydrogen. All these numbers may, roughly it is true, be united under a single law: thus, $\frac{\mu-1}{d}$ for hydrogen $=15696$, its logarithm is 19578, and half this is ·09789 ; this is according to Prof. Everett's data. Mr. Lupton's give ·09374. If we multiply these successively by 1, 2, . . . and 10, we shall find that almost all the logarithms range between these two products, being less than the greater and greater than the smaller. It is worth noticing that $\frac{\pi}{2} = 1·57079$, $\log \frac{\pi}{2} = 19612$—a curious coincidence which, if quite fortuitous, will nevertheless prove a help to the calculator.

I have not as yet attempted to express these relations under an algebraic formula, although it is obvious that it might be very readily done I prefer to call attention to the existence of these empirical relations, which hitherto seem to have escaped notice.

In some of the instances set forth in the Tables the density was checked by calculation from the equivalents. This also, it appears, opens another field of great interest. The well-known relations between molecular weight and density lead us to expect a relation between molecular weight and refraction, and this relation has been abundantly worked out in the case of liquids by my friend Dr. Gladstone. I have not as yet had time to work out this part of the subject completely, as though the calculations necessary present no difficulty they require a numerous and bulky array of figures which must be carefully verified and checked, and also because more data are needed. It is also unfortunate that no data exist, which are accessible to me, of the absorption-spectra of chlorine, bromine, or iodine, or of sulphur and selenium, comparable with the observations here used as the basis of the calculations in the Table. It is in the hope that some of these data may be supplied that I venture to put this paper before the Society.

Some interesting relations are observable between the equivalents and refractions* of gaseous elements and compounds, which I hope to present in a future paper.

Note.—The data are taken from Prof. Everett's 'Units and Physical Constants,' 2nd ed. 1886 (marked E); and from Mr. Lupton's Numerical Tables, 1884 (marked L).

I. Substance.	II. Refraction, $\mu-1$.	III. Density. *d*.	IV. Sp. Ref. $\frac{\mu-1}{d}$.	V. Ratio of Refraction to that of Hydrogen.	Remarks.
H.	0001387 14208	00008837 94630	15696 E 19578	1·00	L 18749 Another value of log $\frac{\mu-1}{d}$ Lupton's data of log.
O.	2706 43233	14107 14943	19182 E 28290	1·95 29025	L 27923 do. $\frac{\mu-1}{d}$
N.	2977 47378	12393 09318	24241 E 38060	2·14 33170	378134 do.
Cl.	L 7720 88762	E 30909 49009	24986 39753	4·27 62992	Density given by Everett.
Cl.	L 7720	L 3·18 50243	24274 38159		Density and Index according to Lupton.
S.	L 16290 1·21192	96×H 98227	19202 28335	11·7 06984	Density according to Gmelin.
P.	L 13640 1·13481	62×H 73869	24895 39612	9·84 99273	Density calculated from equivalent.
Hg.	L 5560 74507	100×H 94630	62918 79877	4·00 60299	Calculated from hydrogen.
As.	L 11140 1·04689	150×H 17609	74268 87080	8·03 90479	Ditto.

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* I propose to call the quantity $\mu-1$ the refraction, μ the index.

Compounds.

I. Substance.	II. Refraction. $\mu-1$.	III. Density. d.	IV. Sp. Ref. $\frac{\mu-1}{d}$.	V. Ratio of Refraction to that of Hydrogen.	Remarks.
N_2O.	E 2975 1·47349	E 19433 28554	15309 18495	2·14 33141	All these are calculated from experimental data of both index and density from Prof. Everett.
NO.	E 5159 71257	E 13254 12235	38924 59022	3·72 33141	
CO.	E 3350 52504	E 12179 29024	22947 36720	3·27 57049	
SO_2	E 7036 84733	E 26990 43120	26070 41613	5·07 70525	
Cy.	E 8216 91466	E 22990 36154	35737 55312	5·92 77258	
NH_3.	385 5855	0·761 8814	7041	2·78 4434	6505 Calculated from Lupton's numbers.
HCl.	449 6522	1·64 2148	4484	3·24 5107	3969
H_2S.	665 8228	1·52 1818	6410	4·79 6807	
CH_4.	443 6464	Calc. 8554	7916	2·02 3040	Density calculated from hydrogen and equivalent.
C_2H_4.	678 8312	Calc. 8554	7328	4·9 6900	

These to four places are evidently of less value than those above.

Note by Prof. A. W. Rücker, *F.R.S., on* Mr. Dale's *Paper.*

It has been shown that the volume of the molecules in unit volume of the substance which they form is $(\mu^2-1)/(\mu^2+2)$, where μ is the refractive index.

If μ is nearly equal to unity (as in the case of the gases) this expression reduces to $\frac{2}{3}(\mu-1)$. Hence if δ is the density of the body, v and m the volume and mass of a molecule and n the number of molecules in unit volume,

$$\frac{\mu-1}{\delta} = \frac{3}{2}\frac{nv}{nm} = \frac{3}{2}\frac{v}{m}.$$

For the same substance the right-hand side should be a constant, and it has been shown that it nearly fulfils this condition.

Mr. Dale now states that for different substances

$$\log\frac{\mu-1}{\delta}=ac,$$

where c is a constant independent of the nature of the substance, and a is an integer. This at once leads to the relation $v/m=\frac{2}{3}e_{ac}$, which would indicate that the ratios of the volumes to the masses of the molecules are in geometrical progression, or, more shortly, the densities of the molecules are in geometrical progression.

If, then, Mr. Dale's conclusions are correct this would be the theoretical inference to be drawn from them.

XXXI. *Notes on Geometrical Optics.*—Part I.

By Professor SILVANUS P. THOMPSON, *D.Sc.*[*]

1. *The Deduction of the Elementary Theory of Lenses and Mirrors from Wave Principles.*

INTRODUCTORY.—The division of Optics into a "geometrical" part, founded upon the treatment of the subject from the ray point of view, and a "physical" part, founded upon the treatment of the subject from the wave point of view, has long seemed illogical. Experience in the teaching of the science suggests that it would be preferable to adopt the wave theory as a common basis, provided the formulæ of lenses and mirrors which form the staple of geometrical optics are as readily established on wave principles as on ray principles. Five years ago the author of these notes made an attempt to rewrite the elementary part of geometrical optics on wave principles; and though hitherto he has published nothing on the subject, he has subjected the method to the test of experience, and has made it the basis of his optical lectures year by year. The form finally adopted, and now for the first time set forth, has been modified from time to time and simplified. As a result, whilst the formulæ for second approximations, aberration and the like, can be deduced with no greater difficulty than in the orthodox way, the elementary formulæ for first approximations are much more simply deduced, and do not even require the use of trigonometrical functions. This is the more striking because, as all teachers of the subject know, the very first assumptions made in establishing the elementary formulæ of spherical mirrors and lenses are that angles may be written instead of their sines or tangents. The assumptions made in the method now published are, it is true, different, but involve no greater sacrifice of accuracy, the formulæ being true to the same degree of approximation, and, indeed, though written in a different notation, are in substance identical with those in common acceptance.

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[*] Read June 8, 1889.

It will be convenient, first, to set forth the general bases of the method; next to define the method of reckoning curvatures; then to explain the notation employed; lastly, to develop the elementary formulæ.

2. *General Bases.*

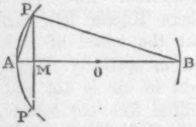
In treating optics from the new standpoint, we have to think about surfaces instead of thinking about mere lines. Waves march always at right angles to their surfaces; a change in the form of the surface alters the direction of march. The wave-surface is to be considered instead of the "ray." The curvature of the surface therefore becomes the all-important consideration. All that any lens or mirror or any system of lenses or mirrors can do to a wave of light is to imprint a curvature upon the surface of the wave. If the wave is initially a plane wave, then the curvature imprinted upon it by the lens or mirror will result in making it either march toward a point (a real focus) or march as from a point (a virtual focus). If the wave possesses an initial curvature, then all that the lens or mirror can do is to imprint another curvature upon its surface, the resultant curvature being simply the algebraic sum of the initial and the impressed curvatures. As will be seen, in the new method the essential thing to know about a lens or mirror is the curvature which it can imprint on a plane wave: this is, indeed, nothing else than what the opticians call its "power;" the focal power being inversely proportional to the so-called focal length. Another, but less vital point in the method, is the abandonment of the use of the so-called index of refraction in favour of a quantity reciprocally related to it, and here denominated the velocity-constant. The use of the index of refraction dates from a time anterior to the discovery that refraction was a mere consequence of the difference of velocity of the waves in different media. The index of refraction is a mere ratio between the sines (or originally the cosecants) of the observed angles of incidence and refraction. The uselessness of clinging to it as a foundation for lens formulæ is shown by the simple fact that, in order to accomplish the very first stage of reasoning in the orthodox way of establishing the formulæ, we abandon the sines and write simply the corresponding angles, as Kepler did before the law of Snell was discovered. The elementary formulæ of lenses are, in fact, where Kepler left them. It is now common knowledge that the speed of light, on which refraction depends, is less in optically dense media than in air. The speed of light in air is not materially different from one thousand million feet per second, or thirty thousand million centimetres per second. If we take the speed of light in air as unity, then the numeric expressing the speed in denser media, such as glass or water, will be a quantity less than unity, and will differ for light of different wave-lengths. It is here preferred to take the speed of light in air, rather than *in vacuo*, as unity, because lenses and optical instruments in general are used in the air. The numeric expressing the relative velocity in any medium is called its "velocity-constant"; it is the reciprocal of the index of refraction. The velocity-constant, for mean (yellow) light, for water is about 0·75; that of crown glass 0·65; that of flint glass from 0·61 to 0·56, according to its density.

3. *Method of Reckoning Curvature.*

The Newtonian definition of curvature as the reciprocal of the radius has a special significance in the present method of treating optics: for some of the most important of lens and mirror formulæ consist simply of terms which are reciprocals of lengths, that is to say of terms which are curvatures. The more modern definition of curvature as rate of change of angle per unit length of the curve (Thomson and Tait's 'Natural Philosophy,' ii. p. 5) is equivalent to Newton's; for if in going along an arc of length δs, the direction changes by an amount $\delta\theta$, the curvature is $\delta\theta/\delta s$. But the angle $\delta\theta = \delta s/r$, where r is the radius of curvature; hence the curvature $= \delta s/r\delta s = 1/r$.

There is, however, another way of measuring curvature, which, though correct only as a first approximation, is eminently useful in considering optical problems. This way consists in measuring the bulge of the arc subtended by a chord of given length.

Fig. 1.



Consider a circular arc AP, having O as its centre. Across this arc draw a chord PP′ of any desired length. The diameter A B bisects it at right angles in M. The short line M A measures the depth of the curve from arc to chord. If the radius is taken as unity the line M A is the versed-sine of the angle subtended at B by the whole chord, or is the versed-sine of the semi-angle subtended at the centre. In Continental works it is frequent to use the name *sagitta* for the length of this line M A; and as this term is preferable to versed-sine, and can be used generally irrespective of the size of radius, it is here adopted. The proposition is that, for a given chord, the *sagitta* is (to a first degree of approximation) proportional to the curvature. For it follows from the construction that

$$\mathrm{MA} \,.\, \mathrm{MB} = (\mathrm{PM})^2;$$

assuming PM as unity,

$$\mathrm{MA} = \frac{1}{\mathrm{MB}} = \frac{1}{2r - \mathrm{AM}}.$$

But, for small apertures, AM is small compared with $2r$, and may be neglected in the denominator, whence

$$\mathrm{MA} \doteqdot \frac{1}{2} \cdot \frac{1}{r}.$$

Twice[*] the sagitta represents numerically the curvature. The error is less than 1 per cent. when the semi-angle subtended at the centre is 10°; less than two per cent. when it is 15°; less than five per cent. when it is 25°.

If the method of reckoning curvatures by means of the sagitta required justification, that is afforded by the fact that the practical method of measuring the curvatures of lenses and mirrors by the *spherometer* consists essentially in applying a micrometer-screw to measure the sagitta of the arc subtended by a fixed chord, the diameter of the contact circle drawn through the three feet of the instrument. In this case, as indeed in all cases where accuracy, not approximation, is desired, the basis for calculation of the correction exists in the actual size of the diameter of the contact circle, which is a fixed parameter for all measurements made with the instrument.

The sign of the curvature remains to be defined. In the case of actual waves of light, the sign adopted will be + for the curvature of waves which are converging upon a real focus; — for those which are diverging either from a luminous source or from a virtual focus. This agrees with the practice of the ophthalmists and of the opticians, who always describe a converging lens as positive. A positive lens is one which imprints a positive curvature upon a plane wave which traverses it.

The *unit of curvature*, whether of the wave-surface itself or of the surface of any mirror or lens, will be taken so as to accord with modern ophthalmic and optical practice as *the dioptrie*; that is to say, the curvature of a circle of one metre radius will be taken as unity. The dioptrie, originally proposed by Monoyer as the unit of focal power of a lens, was formally adopted in 1875 by the International Medical Congress at Brussels, and its great convenience has led to its universal adoption for the enumeration of the focal powers of lenses. That lens which has a focal length of 1 metre is said to have a focal power of one dioptrie. In other words, such a lens prints a curvature of one dioptrie upon a plane wave which is incident upon it. For the present proposal to extend the use of the term from focal powers (*i. e.* imprinted wave-curvatures) to the curvatures of curved surfaces in general, the writer is responsible.

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[*] Though the sagitta is numerically *half* the curvature, since all the formulæ of first approximation are homogeneous and of the first degree as regards sagittæ and curvatures, the numerical factor ½ disappears in passing from sagittæ to curvatures, or *vice versâ*.

4. *Notation.*

In adopting a notation which embodies the new method it is obviously advisable to choose one which lends itself most readily to the existing and accepted notations. In the great majority of books on optics, the recognized symbol for focal length is f; that for radius of curvature r. And in the Cambridge text-books for many years the distances from lens or mirror of the point-object and the point-image have respectively been designated by the letters u and v. Now it is the reciprocals of these which occur in the expressions for the curvatures of surfaces or of waves. The symbols adopted respectively for the four reciprocals are accordingly $\mathcal{F}$, $\mathcal{R}$, $\mathcal{U}$, and $\mathcal{V}$. The accepted symbol for the index of refraction is the Greek letter μ; for the velocity-constant, which is its reciprocal, we take the letter h. The following is a tabular statement of the symbols and their meanings:—

Symbol.	Meaning.	Equivalent in Current Notation.
$\mathcal{F}$	Focal curvature, or Focal power of lens or mirror (= dioptries, if metre is taken as unit of length)	$\frac{1}{f}$
$\mathcal{R}$	Curvature of Surface..................................	$\frac{1}{r}$
$\mathcal{U}$	Curvature of Incident wave; *i. e.* curvature which it has acquired by having travelled from point of origin ("incident focus") to incidence.......................................	$\frac{1}{u}$
$\mathcal{V}$	Curvature of Resultant wave; *i. e.* curvature with which wave emerges from the lens......	$\frac{1}{v}$
h	Velocity-Constant of medium; *i. e.* velocity of light in that medium compared with velocity in air taken as unity	$\frac{1}{\mu}$

Expansion of Curvatures.—If the curvature $\mathscr{R}$ of a wave at any point is known it is easy to calculate the curvature at any other point at distance d further from or nearer to the centre, the formula for the new curvature $\mathscr{R}'$ being as follows:—

$$\mathscr{R}' = \mathscr{R}\frac{1}{1 \pm \mathscr{R}d} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

The + sign must be taken where the new point is further from the centre than the point for which the curvature $\mathscr{R}$ is specified; the − sign when it is nearer the centre. This proposition is of use in dealing with thick lenses, and with thin lenses at a given distance apart.

5. *Refraction Formulæ.*

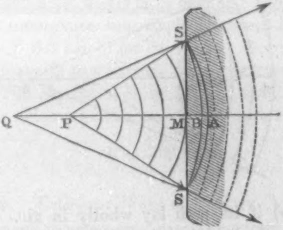
As a preliminary to lens formulæ, it is convenient to consider certain cases of refraction.

A. PLANE SURFACE: CURVED WAVE.

Case (i.) *a. Entrant Wave of Negative Curvature.*

Consider a retarding medium, such as glass, bounded on the left (fig. 2) by a plane surface S S. Let P be a source of waves incident on the surface, PM being a line perpendicular to S S. The wave-fronts, at successive small intervals of time, are represented by arcs of circles. At a certain moment the wave, had it been going on in air, would have had for its surface the position SAS; the curvature being measured by the sagitta AM. The medium, however, retards the wave,

Fig. 2.



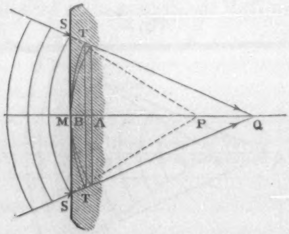
and it will only have gone as far as B instead of penetrating to A; B being a point such that BM $= h$. AM, where h is the velocity-constant of the medium into which the wave enters. The curvature of the wave is flattened as the result of the retardation. Now draw a circle through SBS, and find its centre Q. To a first degree of approximation the arc SBS represents the retarded wave-front, the set of wave-fronts from B onwards being represented by the series of arcs drawn from Q as centre. An eye situated in the medium on the right of SS will perceive the waves as though coming from Q, the (virtual) point-image of P. Accurately the wave-fronts should be hyperbolic arcs, but if S S is small relatively to P M the circular arcs are adequate. Now A M $= \mathcal{U}$, and B M $= \mathcal{V}$. Hence the action of the plane surface upon the curvature of the incident wave is given by the formula

$$\mathcal{V} = h\mathcal{U}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Case (i.) *b*. *Entrant Wave of Positive Curvature.*

The entrant wave (fig. 3) has a positive curvature or convergence such as would cause it to march to the point P (the incident focus) if its path lay wholly in air. At a certain moment, when the middle point of the wave-front has reached M, the outer portions of the wave-front passing in through S S would have reached positions as far as the vertical line drawn through A had the path lain wholly in air. But being retarded they only reach as far as the line T B T drawn through B; where again B M $= h$. AM. The circular arc through T B T has Q for its centre; that is to say, after

Fig. 3.



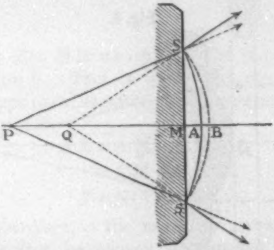
entry the waves now converge on Q instead of P. In this case also the effect on the wave of entrance into the retarding medium is to flatten its curvature, and the formula is as before,

$$\mathcal{V} = h\mathcal{U}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2)\ bis.$$

Case (ii.) *a. Emergent Wave of Negative Curvature.*

Consider the wave emerging (fig. 4) into air from a point P, situated in the retarding medium whose velocity-constant is h. Had the wave been going on wholly through the denser medium, the wave-front would have been at S A S; but, being accelerated on emergence into air, it reaches B instead of A. The new curve S B S has Q for its centre; that is to say, the wave emerges from Q as a virtual focus, its curvature being augmented. The sagitta B M is greater than A M in the ratio of 1 to h. Hence in this case the formula is

Fig. 4.



$$\mathcal{V} = \frac{1}{h}\mathcal{U}. \quad . \quad . \quad . \quad . \quad . \quad . \qquad (3)$$

Case (ii.) *b. Emergent Wave of Positive Curvature.*

There is no need to prove this case separately; it leads to the same formula

$$\mathcal{V} = \frac{1}{h}\mathcal{U}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (3)\ bis.$$

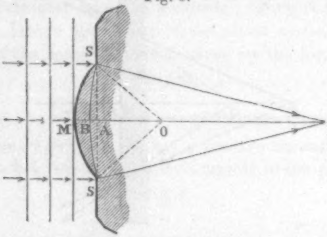
In the case of either positive or negative initial curvature, emergence from the retarding medium through the plane surface into air augments the curvature.

B. Curved Surface : Plane Wave.

Case (i.) *a.* *Entrant Wave ; Convex Surface.*

The surface S M S is convex toward the light ; the centre of curvature being O. Consider the plane wave entering the surface. At a certain instant it would have arrived at S A S had its path lain wholly in air. Because of the retarding medium the central portion of the wave-front will only reach B instead of A ; B being such a point that B M $= h$. A M. As a result the wave is imprinted with the curved surface S B S, the centre of which is at F, toward which point the wave converges. The sagitta A B measures the focal curvature thus impressed on the wave. Now

Fig. 5.



$$AB = AM - BM$$

$$= AM - h \,.\, AM,$$

or

$$\mathscr{F} = \mathscr{R}\,(1-h). \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

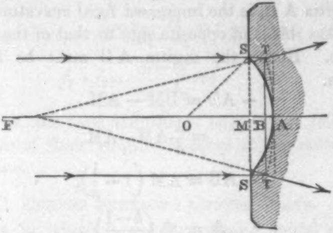
It will be noted that if the curvature of the surface is positive (*i. e.* bulging toward the source of light), the impressed focal curvature is also positive. For the case of any two media having respective velocity-constants h_1 and h_2, the formula becomes

$$\mathscr{F} = \mathscr{R}\frac{h_1 - h_2}{h_1}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Case (i.) *b.* *Entrant Wave ; Concave Surface.*

Consider the plane wave which would have reached the vertical line though A had its path lain wholly in air ; the central portion does reach A, but the marginal portions are retarded, and only reach the vertical line T B T drawn through B, a point such that $BM = h \,.\, AM$. The impressed curve is T A T with centre F. That is to say, the concave (or negative) surface imprints a negative focal curvature on the wave, its sagitta being A B.

Fig. 6.



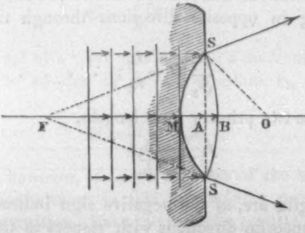
$$
\begin{aligned}
AB &= AM - BM \\
&= AM - h \,.\, AM, \\
\mathcal{S} &= \mathcal{R}(1-h). \quad . \quad . \quad . \quad . \quad . \qquad (4)\ \textit{bis}
\end{aligned}
$$

The formula, therefore, is the same for entrant plane waves whether the surface be convex or concave, the sign of $\mathcal{S}$ following the sign of $\mathcal{R}$.

Case (ii.) *a. Emergent Wave; Surface Convex toward light* (*i. e.* concave toward air into which wave emerges).

The plane wave would have had its front at S A S (fig. 7) at a certain instant had its path lain wholly in the retarding

Fig. 7.



medium; but the central portion being accelerated by its emergence at M into air reaches B, where B M is to A M as 1 to h. Hence the curve S B S, whose curvature is measured by the sagitta A B, is the impressed focal curvature. It will be noted that this is of opposite sign to that of the surface of emergence. Hence the sagitta A B must be taken with minus sign.

$$-\mathrm{AB} = \mathrm{BM} - \mathrm{AM}$$

$$= \frac{1}{h}\mathrm{AM} - \mathrm{AM}$$

$$+\mathrm{AB} = \mathrm{AM}\left(1 - \frac{1}{h}\right),$$

$$\mathcal{F} = \mathcal{R}\left(\frac{h-1}{h}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

As before, for any two media having respective velocity-constants h_1 and h_2, the formula becomes

$$\mathcal{F} = \mathcal{R}\frac{h_1 - h_2}{h_1}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)\ bis.$$

which, in the present case where $h_1 < h_2$, will give $\mathcal{F}$ of opposite sign to $\mathcal{R}$.

Case (ii.) *b. Emergent Wave; Surface Concave toward light* (*i. e.* convex toward air into which wave emerges).

This is similar to the preceding, and yields the same formula.

Comparison of Case (i.)*a with Case* (ii.)*b.*

Comparing formula (4) with formula (6), we get for the two primary focal curvatures impressed respectively on plane waves passing in opposite directions through the curved surface,

$$\frac{\mathcal{F}_1}{\mathcal{F}_2} = -\frac{h_2}{h_1}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

whence, for the two primary focal lengths,

$$\frac{f_1}{f_2} = -\frac{h_1}{h_2}.$$

The focal lengths are, as the negative sign indicates, to be measured in opposite directions with respect to the surface. Also, taking the algebraic sum of the two primary focal lengths, we get

$$f_1 = r\frac{h_1}{h_1-h_2},$$

$$f_2 = r\frac{h_2}{h_2-h_1} = r\frac{-h_2}{h_1-h_2},$$

$$f_1+f_2 = r\frac{h_1-h_2}{h_1-h_2} = r:$$

and, as one of the focal lengths is negative, it follows that the difference of their lengths is equal to the radius of the curved surface.

C. Curved Surface: Curved Wave.

The cases in which a wave possessing initial curvature passes through a curved surface and acquires a resultant curvature may be dealt with, apart from any further geometrical constructions, by applying the principle of superposition of curvatures. Thus, take the case of a wave possessing initial curvature $\mathcal{U}$ entering from air into a medium having velocity-constant h, and so curved that the focal power of the curved surface is $\mathcal{F}$. Then, as the wave enters the surface of the medium two effects will occur: its initial curvature will be altered in the ratio of the velocities, and there will be superposed upon it the focal curvature of the surface; or, in symbols,

$$\mathcal{V}_1 = h\mathcal{U} + \mathcal{F}_1. \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

For an emergent wave, possessing initial curvature $\mathcal{U}$ in the medium, the formula will be

$$\mathcal{V}_2 = \frac{1}{h}\mathcal{U} + \mathcal{F}_2. \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

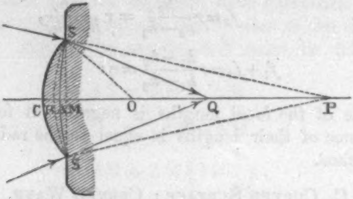
Or, for the case of a wave passing from a medium of velocity-constant h_1 to another of velocity-constant h_2, the formula will be

$$\mathcal{V} = \frac{h_2}{h_1}\mathcal{U} + \mathcal{F}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

It is easy, however, to prove any one of the several cases that may arise, without in this way relying upon the principle of superposition. Take the case of a positive wave entering a positively curved surface.

Let S C S (fig. 8) be the surface of the medium, its curvature being measured by the sagitta C M. There will be a certain moment when the entrant wave, converging toward P, would have had as its front S A S had its path lain wholly in air. But the central portion has entered the retarding medium at C, hence will only have advanced as far as B instead of A; B being such that $BC = h . AC$. Hence the resultant wave will have the form S B S, and the sagitta of the resultant curvature is B M.

Fig. 8.



$$\begin{aligned} BM &= CM - CB, \\ &= CM - h . AC, \\ &= CM - h(CM - AM), \\ &= CM(1-h) - h . AM; \end{aligned}$$

$$\mathcal{V} = \mathcal{R} - h\mathcal{R} + h\mathcal{U}.$$

But

$$\mathcal{R}(1-h) = \mathcal{F},$$

whence

$$\mathcal{V} = \mathcal{F} + h\mathcal{U}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)\,bis$$

D. Lens Formulæ.

Case i. *Thin Lens; Plane Wave.*

In the case of a lens, the curvature $\mathcal{F}_1$ imprinted on a plane wave by entrance at the first surface may be regarded as an initial curvature of the wave which emerges through the second surface. Emergence into air will, as shown above, alter the curvature by augmenting it in the ratio of 1 to h, and superpose upon it the focal curvature $\mathcal{F}_2$ due to the second surface. Hence the whole resultant curvature $\mathcal{F}$

imprinted by a *thin* lens on the plane wave will be

$$\mathscr{S} = \frac{1}{h}\mathscr{S}_1 + \mathscr{S}_2$$

But

$$\mathscr{S}_1 = \mathscr{R}_1(1-h),$$

and

$$\mathscr{S}_2 = -\mathscr{R}_2\left(\frac{1-h}{h}\right);$$

whence

$$\mathscr{S} = \mathscr{R}_1\frac{1-h}{h} - \mathscr{R}_2\frac{1-h}{h},$$

or

$$\mathscr{S} = (\mathscr{R}_1 - \mathscr{R}_2)\frac{1-h}{h}. \quad . \quad . \quad . \quad . \quad (11)$$

This formula may be compared with that in the current notation,

$$\frac{1}{f} = \left\{\frac{1}{r_1} - \frac{1}{r_2}\right\}(\mu - 1).$$

In the case of a lens composed of a medium h_2, lying between two other media h_1 and h_3, the formula becomes

$$\mathscr{S} = \frac{1}{h_1 h_2}\{\mathscr{R}_1(h_1-h_2)h_2 + \mathscr{R}_2(h_2-h_3)h_1\}. \quad . \quad (12)$$

Case ii. *Thick Lens; Plane Wave.*

If the lens has thickness d, the rule for expansion of curvature at end of § 4 above at once gives

$$\mathscr{S} = \mathscr{S}_2 + \frac{1}{h}\mathscr{S}_1\frac{1}{1 \pm \mathscr{S}_1 d}, \quad . \quad . \quad . \quad . \quad . \quad (13)$$

or

$$\mathscr{S} = \left\{\mathscr{R}_1\frac{1}{1 \pm \mathscr{R}_1(1-h)d} - \mathscr{R}_2\right\}\frac{1-h}{h}. \quad . \quad (14)$$

Case iii. *Any Lens; Any Wave.*

The principle of superposition at once gives the universal formula for all lenses bounded by identical media on the two sides :—

$$\mathscr{V} = \mathscr{U} + \mathscr{S}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

or, in words, the resultant curvature is the algebraic sum of the initial curvature and the impressed curvature. This may again be compared with the formula in current notation:

$$\frac{1}{v} = \frac{1}{f} - \frac{1}{u}.$$

The difference in sign attributed to the term $\frac{1}{u}$ arises from conventions adopted in the two systems.

E. Two Thin Lenses at distance apart.

The principle of expansion of curvature at once gives us as the equivalent focal power,

$$\mathscr{F} = \mathscr{F}_2 + \mathscr{F}_1 \frac{1}{1+\mathscr{F}_1 d}, \quad . \quad . \quad . \quad . \quad . \quad (16)$$

where $\mathscr{F}_1$ and $\mathscr{F}_2$ are the focal powers of the first and second lenses, and d the distance between them. $\mathscr{F}$ will be in *dioptries* if $\mathscr{F}_1$ and $\mathscr{F}_2$ are in *dioptries* and d in metric units. If the two thin lenses are close together, the resultant power is simply the algebraic sum of the powers of the separate lenses. One simply adds the *dioptries* of the separate lenses to find the resultant *dioptries*.

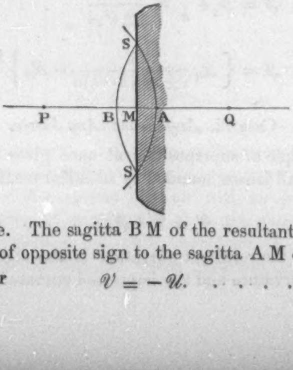
6. *Reflexion Formulæ.*

Mirrors.

Case i. *Plane Mirror; Curved Wave.*

The mirror (fig. 9) has surface S M S. The incident wave would have had front S A S at a certain instant had its path lain wholly in air. The central portion of the wave, which would have reached A, travels backwards to B, an equal distance, in the same time. The sagitta B M of the resultant curvature is equal to and of opposite sign to the sagitta A M of the initial curvature; or

Fig. 9.

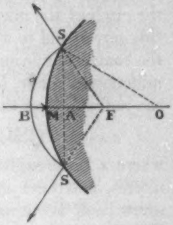


$$\mathscr{V} = -\mathscr{U}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

Case ii. *Curved Mirror; Plane Wave.*

There are two cases, equally simple, of convex and concave mirrors. One will suffice. Consider (fig. 10) a plane wave which at a certain instant would have arrived at S A S had its path lain wholly in air. The central portion of the wave has, however, struck at M, and marches backwards to B in same time as it would have taken to reach A. Hence

Fig. 10.



$$BM = AM,$$

or

$$BA = 2AM.$$

But A M measures the curvature of the mirror, whilst B A measures the curvature impressed on the plane wave. Hence

$$\mathcal{F} = 2\mathcal{R}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

Case iii. *Any Mirror; Any Wave.*

The principle of superposition at once leads to a general formula, expressing the sum of the two actions of the mirror on the wave; it reverses its initial curvature, and then imprints a focal curvature upon it. In symbols,

$$\mathcal{V} = -\mathcal{U} + \mathcal{F}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

7. *Formula of the Lenticular Mirror.*

The lenticular mirror consists of a lens one face of which is silvered, so that the light passes twice through the lens, and suffers an intermediate reflexion at the silvered surface. If the focal power of the lens be $\mathcal{F}_1$, and the curvature of the mirror-surface be $\mathcal{R}$, then the total curvature which the system impresses upon a plane wave will be equal to $2\mathcal{F}_1 + 2\mathcal{R}$, and there will be a reversal of the direction of the light, so that

$$\mathcal{F} = -2(\mathcal{F}_1 + \mathcal{R}). \quad . \quad . \quad . \quad . \quad . \quad (19)$$

8. *A Dioptric Spherometer.*

In order the better to bring to practical issues the method of treating lens problems by curvatures, the author designed a special spherometer. In this instrument the readings of the scale are caused to indicate directly the dioptries of the curvature; that is to say, one whole turn of the micrometric screw corresponds accurately to a curvature having one metre as its radius. In the particular instrument now exhibited the pitch of the micrometric screw is 1 millimetre. To make this correspond to a curvature of one dioptrie, the radius of the contact-circle on which the three feet stand must be 44·71 millimetres. For, by reference to fig. 1, it will be seen that the radius PM of the contact-circle is such that

$$(\mathrm{PM})^2 = \mathrm{AM} \,.\, \mathrm{MB} = 2rs - s^2,$$

where s is the sagitta of the curve. Hence, if $r = 1000$ millimetres, and $s = 1$ millimetre, $\mathrm{PM} = \sqrt{1999} = 44{\cdot}7102$. The screw itself is cut on a steel tube 8 millimetres in diameter. The tips of the three feet and of the central point are of phosphor-bronze, which does not scratch glass. The arms carrying the three feet are girder-shaped, thin, deep; so as to be very firm vertically; lateral flexure is immaterial. The tips of the three feet are adjusted by filing to chisel-shaped edges, forming little tangents to the imaginary circle of contact. The divided circle is not, as ordinarily, cut upon a flat plate, but is cut upon a thin cylinder of aluminium-bronze affixed to the screw by a light spider with three arms. The whole instrument weighs only 78 grammes. It was constructed for the author by Messrs. Nalder Brothers.

The formula for the focal power of any lens (formula no. (11), p. 207) consists of two factors—one depending solely on the shape of the lens, the other upon its material. The latter factor, $\frac{1-h}{h}$, is a mere numeric; whilst the former, being the difference of two curvatures, is itself a curvature. If the curvature thus determined by shape solely is expressed in dioptries, then, on multiplying by the numeric which depends on the nature of the material, the resultant power of the lens will also be expressed directly in dioptries. The dioptrie spherometer facilitates this desirable end by its directness of reading, quite apart from any incidental advantages which may result from the details of construction.

XXXII. *Researches on the Electrical Resistance of Bismuth.*

By Edmond van Aubel, *Doctor of Science*[*].

A great many memoirs having been published relating to the influence of temperature upon the electrical resistance of bismuth, I think it advisable to sum up in a few lines the actual state of the question, in order to show the bearing of my researches on the solution of the problem.

In the present memoir I have examined the electrical conductivity of bismuth between 0° and 100° only; I will therefore confine myself to stating the results arrived at by physicists between the same limits of temperature.

Matthiessen[†], in conjunction with von Bose and other physicists, has found the conductivity of bismuth at 0° to be 1·245, that of silver being taken as 100, and has expressed the conductivity at t° as a function of the conductivity at 0° by the formula

$$\lambda_t = \lambda_0 (1 - 0{\cdot}0035216\,t + 0{\cdot}000005728\,t^2).$$

G. Wiedemann, Matthiessen, Holzmann, and Vogt have also studied the effect of the composition of the alloys Bi–Sn, Bi–Pb, upon their electrical conductivity.

According to Matthiessen and Vogt, the electrical conductivity of the alloys of bismuth is modified by the first heating and the first cooling. If λ_0 be the original value, and λ_{01} the conductivity after cooling, we have the following results:—

	λ_0.	λ_{01}.
Pb–Bi (2·27 vol. Pb to 100 vol. Bi) .	8·101	7·633.
„ (18 „ „ „) .	4·558	4·565.

These variations doubtless depend upon permanent changes in the molecular structure.

The conductivity of molten bismuth increases as the temperature is lowered; it diminishes rapidly when the metal solidifies, according to the researches of Matteucci and Matthiessen, and increases again as the solidified mass cools. If small traces of tin or lead be added to the molten bismuth, according to Matthiessen, the conductivity at first diminishes, as in the case of solid metals, and afterwards increases.

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[*] Read June 22, 1889.

[†] The works of the physicists, whose names will be mentioned later on, are enumerated in the Treatise on Electricity by Prof. G. Wiedemann, vol. i. 1882, p. 503 and the following pages.

Fr. Weber has found for the specific electrical conductivity of bismuth (C.G.S.) $0{\cdot}838 \times 10^{-5}$; and L. Lorenz has given for the same quantity the values

$$0{\cdot}929 \times 10^{-5} \text{ at } 0^\circ; \text{ and } 0{\cdot}630 \times 10^{-5} \text{ at } 100^\circ.$$

M. Leduc[*] has observed that the electrical resistance of wires made of an alloy of bismuth and lead increases when the temperature is raised. He has also observed a difference between the electrical properties of wires and of thin plates of commercially pure bismuth, which he attributes to the method of preparation and to the very different rates of cooling.

More recently[†] the same physicist has found the following results with bismuth which had been run into a tube and then slowly cooled. When the bismuth is heated for the first time, say to 100°, it undergoes an annealing the effect of which is to diminish its initial resistance about 30 per cent. During this operation, between the temperatures 0° and 100°, the following formula is obtained, which must be received with caution:—

$$r_t = r_0(1 + 0{\cdot}00344t + 0{\cdot}0000077t^2),$$

in which r_0 and r_t are the electrical resistances at the temperatures 0° and t° respectively. This formula gives the value $+0{\cdot}00421$ for the mean coefficient of variation of the resistance between 0° and 100°.

Afterwards the resistance varies, between the same limits, according to the formula:—

$$r_t = r_0(1 + 0{\cdot}00375t + 000008t^2),$$

and the mean coefficient of variation of the resistance between 0° and 100° is then 0·00455.

For a thin plate of bismuth, Leduc has found that after annealing the resistance decreases, between 0° and 70°, according to the following formula:—

$$r_t = r_0(1 - 0{\cdot}00158t + 0{\cdot}00000437t^2).$$

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[*] *Journal de Physique,* [2] iii. (1884) p. 362.

[†] Leduc, Thesis for doctor's degree presented in June 1888 to the Faculty of Sciences of Paris.

And this gives the number −0·00127 for the coefficient of variation with the temperature between 0° and 70°. The resistance of the metal thus prepared would be much greater than that given by Matthiessen, perhaps more than double the value found by him, no doubt because of the difference of molecular structure of the specimens examined.

In 1884, Prof. Righi [*] published a very remarkable paper, and we will sum up those of his conclusions which bear directly upon our researches.

1. The resistance of commercial bismuth increases on heating between certain limits of temperature, and decreases between others (generally it decreases at temperatures near the ordinary temperature), and, by constructing a curve of resistance, with the temperatures as abscissæ, and the specific resistances as ordinates, we obtain in general a curve in the form of an M. There is a maximum at a low temperature, then a minimum, then a second maximum a little below fusing point, and finally a second minimum after the change of state.
2. The resistance of commercial bismuth varies not only with the temperature, but also with the manner in which the piece has been prepared, and with the temper of the metal.
3. Chemically pure bismuth behaves like other metals; it is not sensibly affected by tempering, and at 0° its resistance compared with that of mercury is 1·15.
4. The difference between pure and commercial bismuth is due to traces of tin, which give to the latter properties similar to those by which steel differs from iron.
5. On adding to bismuth tin in increasing amounts, the specific resistance becomes much greater up to a maximum, and then diminishes.
6. The presence of tin in increasing amounts modifies the curve of resistance, in the same manner as a temper more and more hard would do.

G. Wiedemann [†] thinks that the phenomena observed by Prof. Righi can be partly accounted for by the discontinuity of the bismuth wires.

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[*] *Journal de Physique*, [2] iii. (1884) p. 355.

[†] *Elektricität*, Bd. iv., ii., p. 1228.

G. P. Grimaldi* has studied the thermoelectric properties of bismuth, and has confirmed the analogies which Prof. Righi has found between pure bismuth and iron, the bismuth containing tin and steel.

A. von Ettingshausen and W. Nernst† have obtained results which are recorded in the following Table, where x is the electrical conductivity in absolute measure, and α the coefficient of variation of the electrical resistance with the temperature. The plates denoted by Bi were made of absolutely pure bismuth; the other plates denoted by LI to LIV were made of an alloy with tin.

	Bismuth.	Tin.	x (G.S.).	α.
	Parts by weight.	Parts by weight.		
Bi	100		$4{\cdot}80\times10^{-6}$	$-0{\cdot}0012$
LI.	99·05	0·95	$2{\cdot}46\times10^{-6}$	$+0{\cdot}0016$
LII.......	98·54	1·46	$2{\cdot}71\times10^{-6}$	$+0{\cdot}0018$
LIII. ...	93·86	6·14	$3{\cdot}46\times10^{-6}$	$+0{\cdot}0024$
LIV. ...	86·9	13·1	$5{\cdot}62\times10^{-6}$	$+0{\cdot}0025$

The electrical resistance increases then (between 0° and 30°) for every alloy, when the temperature is raised; it decreases on the contrary for pure bismuth under the same conditions. On increasing the quantity of tin, the coefficient of temperature α also increases. The conductivity decreases rapidly by the addition of small quantities of tin, and increases again afterwards.

C. L. Weber‡ has found that the resistance of the metal under consideration at first decreased up to about 100°, the coefficient of temperature being $-0{\cdot}0006$; and then increased up to the melting-point. The position of the minimum, however, is displaced by repeated heatings and coolings between 80° and 120°.

The same physicist has also observed that the electrical resistance of alloys of bismuth and tin, containing from 10 to 80 per cent. of bismuth, increases between 0° and 120°, as the temperature is raised.

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* *Beiblätter zu den Annalen der Physik* (1889), No. 1, p. 25.

† W. Nernst, *Annalen der Physik*, Neue Folge, Bd. xxxi. p. 783 (1887); A. von Ettingshausen and W. Nernst, *Annalen der Physik* (1888), Heft iii. p. 474.

‡ *Annalen der Physik*, 1888, xxxiv. p. 576.

In his *Leçons sur l'Eléctricité*, Prof. Exner[*] mentions that when bismuth is heated the resistance generally decreases, and does not return to its original value on cooling, but reaches a value which is higher as the cooling is more gradual.

Ph. Lenard and J. L. Howard[†] have studied bismuth wires obtained by means of a screw-press; these wires were rolled into a spiral immediately, while the metal was still hot. They found that in the case of pure bismuth the electrical resistance increased with the temperature between 0° and 36° by 0·0052 for every degree Centigrade.

Finally, in a preliminary communication, published last year[‡], I took up again the study of the question. Although bought at the best manufactories in Germany, all the bismuth that I used was very impure.

I examined bismuth slowly cooled, hardened bismuth, and finally compressed bismuth. The molecular structure exerts a great influence upon the electrical properties of different kinds of impure bismuth. All the alloys of bismuth and tin which I studied gave an increase of electrical resistance when the temperature was raised, although the bismuth specimens which entered into the composition of these alloys produced the opposite effect. I also proved, in these preliminary experiments, the great influence of lead as an impurity in bismuth.

If my first results and those of the other physicists be compared with the conclusions arrived at in my present treatise, the wide differences which can be caused by impurities in the pieces of metal under examination will be very striking.

The modes of preparing and of suddenly cooling rods of bismuth have been fully described in a preliminary communication[§].

Analysis of different Bismuths.

We have measured the electrical resistances of several different bismuths, which we will designate by

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[*] *Vorlesungen über Elektricität*, Wien (1888), p. 404.

[†] *Elektrotechn. Zeitschrift*, 1888, Bd. ix., July, Part xiv.

[‡] *Bulletins de l'Académie royale de Belgique*, 1888, 3rd Series, xv. No. 1 (Preliminary communication).

[§] Phil. Mag. vol. xxv. p. 191; Proc. Phys. Soc. Lond. vol. ix. p. 124.

Latest Brommsdorff,
Classen I.,
Classen II.,
Classen III.,
Classen IV.,
and pure electrolyzed bismuth.

The first is the metal as pure as it is possible to obtain it commercially. It has been supplied to us by the well-known chemical works of Herr Brommsdorff at Erfurt, who has taken all possible care in its preparation. We have so named this product to distinguish it from Herr Brommsdorff's other bismuths, which we treated of in our "Preliminary Communication."

The qualitative analysis of this metal, made by Prof. Classen, showed that it contains several impurities, principally copper.

The four following bismuths are products as pure as can be obtained by chemical methods of precipitation; they were prepared by Prof. Classen, who used every precaution. The metals Classen I., II., and III. were obtained by reduction from bismuth oxychloride, Classen IV. by reduction from bismuth nitrate.

Bismuth nitrate is a pharmaceutical product which, it appears, can be obtained in a very pure state in commerce.

These different samples were subjected to a minute spectral analysis, the results of which we will point out later, but we may mention here that they all contained lead. Small quantities of lead were always carried down with the precipitated bismuth; this experiment repeated even 13 times never resulted in a pure product. It appears then that *bismuth cannot be obtained absolutely pure by precipitation.*

The last bismuth which we examined, and which we have called "pure electrolyzed bismuth," was prepared by electrolysis. During the electrolysis of a solution of bismuth containing traces of lead, pure bismuth was deposited at the negative pole; and lead, in the form of peroxide of lead, at the positive pole.

As to the lead or tin which were used in the alloys, they came from Herr Brommsdorff's chemical works, and were sold to me as pure. Besides, they were introduced into the alloys in such small quantities that there was no occasion to take account of any impurities they might have contained.

Purification of Commercial Bismuth.

First Method.

About 250 grams of the metal were dissolved in HNO_3; the solution was brought to the boiling-point in a porcelain dish into which concentrated HCl had been poured. In order to transform all the bismuth nitrate into the chloride, the operation was repeated until the presence of HNO_3 could no longer be detected. The residue was then dissolved in HCl, and alcohol added in successive small quantities. Most of the lead was precipitated in the form of lead chloride, which was removed by filtration. The solution of bismuth was then distributed among 10 glass beakers holding about 4 litres each. The bismuth was precipitated as oxychloride on addition of water. The precipitate was decanted and washed until no trace of HCl could be found. Then it was again dissolved in HCl, the bismuth precipitated as oxychloride, and washed as before. And this was done twelve times. The precipitate finally obtained was carefully washed again, then dissolved in HCl. The bismuth in this solution, to which water had been added, was precipitated by the addition of ammonia and ammonium carbonate. After decantation the precipitate was washed with water, until all the ammonia had disappeared, and dissolved in HCl. This precipitation was repeated three times.

Finally, the precipitate was dissolved in HCl, and the chloride transformed into the oxychloride by addition of water. The resulting precipitate, entirely freed from acid, was dried, mixed with KCN, and reduced. The metallic bismuth was again purified by a second fusion with KCN.

The bismuths Classen I. and Classen III. were both prepared under the direction of Prof. Classen in the laboratory for analytical chemistry at the Polytechnical School at Aix-la-Chapelle.

Classen I. was prepared by Herr Nörrenberg; Classen III.

was obtained by Herr Magdeburg by means of the "Bismuth Purissimum" from the Schucharell works at Görlitz.

Finally, we are indebted to the kindness of Prof. Classen himself for Classen II.

Second Method.

The product used in this second method was the "bismuth subnitrate purissimum" of Dr. Marquardt, at Bonn, which was employed by Marignac in his determination of the atomic weight of bismuth.

One kilogram of this product was dissolved in HCl, and the solution was divided among 22 glass beakers, having a capacity of 4 litres each, and these were filled with water. The precipitate of oxychloride was washed with water until all the HCl was removed. The solution in HCl and the precipitation by means of water were repeated three times. Then the precipitate was again dissolved in HCl, precipitated with ammonia and ammonium carbonate, and thoroughly washed with water. These operations were also repeated three times.

Finally, the precipitate was once more dissolved in HCl, the bismuth oxychloride obtained by the addition of water, and reduced by means of KCN and soda.

Third Method.

The metal, after having been purified by the preceding methods, was subjected to electrolysis to take away completely any trace of lead. Prof. Classen, who is at present engaged in determining the atomic weight of bismuth, intends to describe this electrolytic method in detail.

Researches with the Spectrum.

To produce the bismuth spectrum a large Ruhmkorff's coil with a Leyden jar intercalated was used, and the spark obtained between rods of the metal. The spectrometer employed was one of Meyerstein's with a Schroeder direct-vision prism composed of five separate prisms. The slit was made rather open so as to give a very bright spectrum.

With a smaller dispersion, produced by a fine Merz prism, the spectrum was still more luminous. It was interesting to compare the method of observation which we have just described with that which consists in sending an electric discharge between platinum electrodes, in a tube containing a solution of a salt of the metal. This method was also used, the rest of the apparatus remaining the same.

It can thus be proved experimentally, and this is of some importance to chemists, *that the first method is much more sure and exact than the other.*

On examining the bismuth "Latest Brommsdorff" in the spectroscope, sodium and copper were found to be present. The two characteristic lines D_1 D_2 of sodium remained, even when the surface of the bismuth rods had been well cleaned by long immersion in nitric acid. The metal contained only traces of lead.

The other bismuths contained no copper and no sodium. The *traces* of lead, which were found in the bismuths Classen I., II., III., IV., and in the "Latest Brommsdorff," were all very faint; indeed we were not able to establish them with certainty, except by the following process:—

After having substituted, for the original rods of bismuth, other rods of lead, so as to produce a good spectrum of this latter metal, the point of cross section of the micrometer wires of the spectroscope was placed upon the most visible line of the lead spectrum, and then the rods of lead were replaced by those of bismuth.

The bismuth Classen I. showed the very characteristic line of lead, of wave-length 5610·4.

The same line was found in Classen II. and Latest Brommsdorff; but it was much more feeble. Classen III. and Classen IV. also contained lead.

To sum up, all these bismuths contained lead in variable amounts, Classen II. and Latest Brommsdorff containing the least.

But in the case of the bismuth obtained by electrolysis, no impurity was discovered by spectrum analysis.

This very minute spectrum analysis of products, prepared with the greatest care by such a distinguished chemist as Prof. Classen, warrants us in saying that *pure bismuth cannot be obtained with certainty by precipitation. The electrolyzed metal can alone be considered as chemically pure.*

In the successive precipitations the traces of lead are drawn down mechanically.

In electrolyzing the solutions of bismuth, in which the spectrum analysis had revealed traces of lead, a very thin but perfectly visible coating of peroxide of lead was found at the positive pole.

Calculation of the Absolute Values of the Electrical Resistances.

The mode of measuring the electrical resistance has been described in the preliminary communication.

In order to determine the absolute value of the electrical resistances at 0° of the tempered rods, it was necessary to calculate the mean section of these rods from their weight, density, and length.

No sensible error is committed in taking 9·82 as the density of bismuth or of the alloys which we have studied, as the lead and tin contained in the latter were only present in very small quantities. Besides, a much greater cause of error is the exact determination of the length of the rods of bismuth between the two solderings.

In the case of the slowly cooled rods the difficulty is greater, because the bismuth adheres very closely to the glass tubes in which it is contained [*], and because these tubes cannot be considered as cylindrical. In spite of every precaution it is impossible to break the glass, so that the whole of the metal may be got out clear without breaking the rod of bismuth. So I measured with a spherometer the diameters of the bismuth rod at the two extremities and in the middle; and I considered the rod as formed of two truncated cones joined together at the small end. It was then easy to calculate the electrical conductivity at 0°; for example, either using the formula given by Siemens (*Annalen der Physik*, vol. cx. 1860, p. 3, or F. Kohlrausch, *Guide de Physique pratique*, édition française, p. 223), or calculating the section of a cylinder whose height is the length of the rod of bismuth, and whose volume is that of the two truncated cones joined together. The section of this cylinder can then be taken as the mean section of the rod.

All the absolute values that I give further on are thus only approximations, but they approach very near to the true values.

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[*] Righi, *Journal de Physique*, 2nd series, vol. iii. 1884, p. 132.

Results of the Electrical Measurements.

The following Tables contain, in column W, the electrical resistances as I have measured them, that is to say in Siemens units, and at different temperatures given by a Centigrade thermometer. I have given also, for each bismuth, the specific electrical resistance R_0 in C.G.S. units at 0° temperature; and the mean coefficient of variation of the electrical resistance between 0° and 100°, *i. e.* the quantity K of the equation $R_t = R_0(1 + Kt)$.

Bismuths.

(1) Rods which have been slowly cooled.

Bismuths.	Temperatures.	W. Electrical resistances in U.S.	K. Variation for 1° between 0° and 100°.	R_0.
Latest Brommsdorff	0°	0·1390	+0·00325	$10^3 \times 109·90$
	19	0·1462		
	49·1	0·1590		
	99·6	0·1840		
Classen I.	0	0·2875	+0·00076	
	19·1	0·2882		
	49·2	0·2922		
	99·6	0·3095		
Classen II..........	0	0·1615	+0·00299	$10^3 \times 124·69$
	16·7	0·1680		
	49·6	0·1828		
	99·5	0·2096		
(2) Classen III. 2nd rod.	0	0·2447	+0·00161	
	20·5	0·2500		
	49·8	0·2595		
	99·8	0·2841		
(1) Classen III. 1st rod.	0	0·2407	+0·00132	$10^3 \times 156·74$
	20·5	0·2439		
	49·7	0·2509		
	99·8	0·2725		
Classen IV. 1st rod.	0	0·2036	+0·00126	$10^3 \times 170·07$
	20·3	0·2063		
	99·8	0·2302		
	It is allowed to cool slowly.			
	0	0·2043		
	19·9	0·2066		
	It is heated to 100°, and allowed to cool slowly.			
	0	0·2048		
	20	0·2069		
	52·5	0·2136		
	99·6	0·2305		
	It is allowed to cool again slowly, and it is found that the values remain constant.			

Table (*continued*).

Bismuths.	Temperatures.	W. Electrical resistances in U.S.	K. Variation for 1° between 0° and 100°.	R_0.
Classen IV. 2nd rod.	0°	0·1790		
	20·2	0·1806		
	99·8	0·1998		
	It is allowed to cool slowly.			
	0	0·1794		
	20	0·1810		
	It is heated to 100°, and allowed to cool very slowly.			
	0	0·1798		
	20	0·1810	+0·00113	$10^3 \times 168·35$
	52	0·1861		
	99·6	0·2000		
	It is allowed to cool again slowly, and it is found that the values remain constant.			
Pure electrolyzed Classen Bismuth, 1st rod.	0	0·1031		
	99·61	0·1490	+0·00447	
	It is allowed to cool very slowly.			
	0	0·1032		
	99·65	0·1491		
	It is allowed to cool again very slowly.			
	0	0·1032	from 0° to 19°·5 : +0·00412	
	19·5	0·1115	from 0° to 55° : +0·00426	
	55	0·1274	from 0° to 99°·72: +0·00447	
	99·72	0·1493		
Pure electrolyzed Classen Bismuth, 2nd rod*.	0	0·0912		
	99·6	0·1322		
	It is allowed to cool very slowly.			
	0	0·0913	from 0° to 22°·1 : +0·00411	
	22·1	0·0996	from 0° to 56° : +0·00426	$10^3 \times 107·99$
	56	0·1131	from 0° to 99°·7 : +0·00450	
	99·7	0·1323		

* We think it will be interesting to give, for the pure electrolyzed bismuth, the values of the influence of magnetism on the electrical resistance.

ΔW being the difference between the electrical resistance in the magnetic field and outside of it.

Temperatures.	$100 \frac{\Delta W}{W}$.	Intensity of magnetic field.
0°	2·9	about 1560 C.G.S. units.
99·7	0·415	

(2) Tempered Rods.

Bismuths.	Temperatures.	W. Electrical resistances in U.S.	K. Variation for 1° between 0° and 100°.	R_0.
Latest Brommsdorff.	0°	0·0965	+0·00199	$10^3 \times 139{\cdot}86$
	17·6	0·0988		
	49·3	0·1040		
	99·7	0·1157		
Classen I. ...	0	0·1745	−0·000603	$10^3 \times 246{\cdot}91$
	17·5	0·1729		
	49·1	0·1687		
	99·7	0·1640		
Classen II. 1st rod.	0	0·1275	+0·00106	$10^3 \times 166{\cdot}66$
	16·7	0·1287		
	49·7	0·1319		
	99·5	0·1410		
Classen II. 2nd rod.	0	0·1292	+0·00128	$10^3 \times 157{\cdot}48$
	16·9	0·1310		
	50·3	0·1353		
	99	0·1456		
Classen II. 3rd rod.	0	0·1205	+0·00116	$10^3 \times 163{\cdot}40$
	18	0·1215		
	99·8	0·1342		
	It is allowed to cool slowly.			
	0	0·1203		
	99·7	0·1343		
	A second observation showed that the resistances now remain constant.			
Classen III. 1st rod.	0	0·1135	+0·00009	$10^3 \times 204{\cdot}50$
	19·7	0·1127		
	99·8	0·1139		
	It is allowed to cool slowly.			
	0	0·1128		
	20	0·1121		
	48	0·1114		
	99·8	0·1138		
	A second set of observations of the electrical resistances from 0° to 100° showed that the values remained constant.			
Classen III. 2nd rod.	0	0·1376	+0·00005	$10^3 \times 208{\cdot}33$
	20	0·1363		
	99·8	0·1372		
	It is allowed to cool slowly.			
	0	0·1364		
	20	0·1355		
	48	0·1346		
	99·8	0·1371		
	A second set of observations showed that the values of the resistances remained constant.			

Table (*continued*).

Bismuths.	Temperatures.	W. Electrical resistances in U.S.	K. Variation for 1° between 0° and 100°.	R_0.
Classen IV. 1st rod.	16·6°	0·1301		
	49·6	0·1285		
	99·7	0·1301		
	It is allowed to cool rather slowly.			
	0	0·1307		
	16·6	0·1295		
	49·3	0·1281	−0·00004	$10^3 \times 207·47$
	99·7	0·1302		
	A second investigation showed that the values of the resistances remained constant.			
Classen IV. 2nd rod.	16·5	0·1507		
	49·6	0·1486		
	99·7	0·1501		
	It is allowed to cool rather slowly.			
	0	0·1512		
	16·5	0·1500		
	49·5	0·1479	−0·00008	$10^3 \times 212·94$
	99·7	0·1500		
	A second investigation showed that the values of the resistances remained constant.			
Pure electrolyzed Classen Bismuth. 1st rod.	0	0·0965	+0·00434	
	99·6	0·1382		
Pure electrolyzed Classen Bismuth. 2nd rod*.	0	0·0952		
	99·6	0·1370		
	It is allowed to cool very slowly.			
	0	0·0950	from 0° to 21°·9 : +0·00399	
	21·9	0·1033	from 0° to 56°·1 : +0·00422	
	56·1	0·1175	from 9° to 99°·7 : +0·00445	$10^3 \times 108·69$
	99·7	0·1372		

* We think it will be interesting to give the value of the magnetic action for this rod of hardened bismuth, as we have done elsewhere.

Temperatures.	$100 \frac{\Delta W}{W}$.	Intensity of magnetic field.
0°	2·89	about 1560 C.G.S. units.
99·7	0·402	

(3) Compressed Rods.

Bismuths.	Temperatures.	W. Electrical resistances in U.S.	K. Variation for 1° between 0° and 100°.	R_0.
Classen II. 1st rod.	20°·6	0·1300		
	It was observed that the electrical resistance was permanently changed each time the rod of bismuth was heated. Finally, after having been heated from 0° to 100°, and been allowed to cool from 100° to 0° several times, constant values were found. We cannot tell what is the cause of this considerable decrease in the resistance, but it is certainly not due to an alteration of the connexions.			
	0	0·1250		
	22	0·1215	−0·00049	10^3 × 236·96
	99·4	0·1189		
Classen II. 2nd rod.	20·5	0·1353		
	Same observation as above.			
	0	0·1352		
	21·9	0·1302	−0·00083	10^3 × 251·26
	99·3	0·1241		
Classen II. 3rd rod.	0	0·1019		
	18	0·0978		
	99·8	0·0913		
	It is allowed to cool slowly.			
	0	0·1024		
	99·7	0·0916	−0·00105	10^3 × 268·10
	A further investigation showed that the resistances then remained constant.			

Alloys with Tin.

Alloys.	Temperatures.	W. Electrical resistances in U.S.	K. Variation for 1° between 0° and 100°.	R_0.
1st. Rods slowly cooled.				
Latest Brommsdorff bismuth + Sn. 0·5 gr. Sn to 100 gr. Bi.	0°	0·2670		
	19	0·2710		10^3 × 458·71
	49	0·2715		
	99·6	0·2623		
Bi Classen II. + Sn. 0·53 gr. Sn to 100 gr. Bi.	0	0·6790		
	16·7	0·6917		10^3 × 416·66
	49·6	0·6890		
	99·5	0·6540		
2nd. Tempered Rods.				
Latest Brommsdorff bismuth + Sn. 0·5 gr. Sn to 100 gr. Bi.	0	0·1960		
	17·7	0·2020		10^3 × 346·02
	49·3	0·2083		
	99·7	0·2095		
Bi Classen II. + Sn. 0·5 gr. Sn to 100 gr. Bi.	0	0·1795		
	17·5	0·1844		10^3 × 354·61
	49·2	0·1905		
	99·7	0·1915		
Bi Classen II. + Sn. 0·53 gr. Sn to 100 gr. Bi.	0	0·2561		
	16·7	0·2637		10^3 × 349·65
	49·9	0·2735		
	99·5	0·2780		

Alloys with Lead.

Alloys.	Temperatures.	W. Electrical resistances in U.S.	K. Variation for 1° between 0° and 100°.	R_0.
1st. Rods slowly cooled.				
Latest Brommsdorff Bi + Pb. 0·5 gr. Pb to 100 gr. Bi.	0°	0·2125		
	19·1	0·2122		
	69·0	0·2127		
	99·6	0·2200		
Bi Classen I. + Pb. 0·5 Pb to 100 gr. Bi.	0	0·4037		$10^3 \times 362·32$
	19	0·3887		
	49·1	0·3690		
	99·6	0·3530		
2nd. Tempered Rods.				
Latest Brommsdorff Bi + Pb. 0·5 gr. Pb to 100 gr. Bi.	0	0·1420		$10^3 \times 245·70$
	17·6	0·1425		
	49·2	0·1400		
	99·7	0·1365		
Bi Classen I. + Pb. 0·5 gr. Pb to 100 gr. Bi.	0	0·1475		$10^3 \times 274·27$
	17·5	0·1485		
	49·1	0·1470		
	99·7	0·1414		

Conclusions.

I have observed that the electrical resistances of some rods were changed permanently after the first heating; but when once the values remain constant they do not change again, even after several months. Dr. Leduc* has observed a similar phenomenon. Rods of the same bismuths, either slowly cooled or hardened under the same conditions, give nearly the same values for the resistance at 0° and for the coefficient of variation with the temperature. The methods of tempering and of slowly cooling remain sensibly the same for the different specimens. We must not then look to this cause to explain the great variations which have been observed between one bismuth and another.

The molecular structure, which I have changed by tempering and by compression, makes a great difference in the electrical properties of impure bismuths. On the other hand, tempering appears to have no action whatever on pure bismuth ; thus, the electrical resistance at 0° is :—

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* Thesis for doctor's degree, p. 30.

for electrolyzed bismuth, slowly cooled . . $10^3 \times 107{\cdot}99$,
and for the same metal, tempered . . . $10^3 \times 108{\cdot}69$.

The coefficient of variation with the temperature and the influence of magnetism are very nearly the same for these two specimens. The coefficient of variation with the temperature besides is *positive*.

In the case of impure bismuths the process of tempering causes the coefficient K to decrease, and even to become negative. The action of compression seems to be still greater.

As to the absolute values of the electrical resistance at 0°, they increase under the action of tempering and compression.

If we compare the results furnished by the electrolyzed bismuth with those given by the other bismuths, we see that the effect of traces of lead is to produce a diminution in the value of K, and an increase in the value of R_0. It can also be seen that, in the case of impure metals, a high value for R_0 generally corresponds to a low value for K.

With regard to alloys of impure bismuth with lead and tin, the results prove that these latter metals tend to increase R_0, and can even in certain cases give a negative value for K. But the molecular action, and above all the lead, produce a much greater effect than the tin.

The electrolyzed bismuth presents a peculiarity which is not seen in the impure kinds. The coefficient K remains sensibly the same at different temperatures between 0° and 100°, and this may be considered as a proof of the purity of the metal.

In short, one may say that, of all the methods both physical and chemical, the determination of the electrical resistance is certainly the most exact for ascertaining if the bismuth be pure, and above all if it contain no trace of lead. While spectrum analysis and the process of electrolysis have with difficulty discovered the existence of lead in the bismuth Classen II. for example, the study of the electrical resistance leads to very different results.

When one considers all the difficulties I have met with in procuring the pure metal, and the number of electrical measurements which I have been obliged to make, so to speak, uselessly, it is plain that, before studying the physical properties of a metal, it would always be wise to submit the metal to a very careful spectrum analysis, to assure one's self of its purity.

The differences, which I pointed out at the beginning, between the results of other physicists are easily explained by the rarity of pure bismuth.

Unfortunately I had not enough of pure bismuth to be able to study compressed electrolyzed bismuth and its alloys with tin and lead.

Considering the great influence of very small traces of lead, it is allowable to suppose that the presence of this foreign metal modifies the molecular structure of the bismuth to a considerable extent; unfortunately a microscopic examination could hardly give any result here.

Finally, if the results which we have obtained in this treatise be compared with those which we published in our "Preliminary Communication," it will be seen how much the conclusions deduced from the experiments may vary according to the nature of the impurities contained in metal. Also, the question of the variation of the electrical conductivity of bismuth with the temperature presents a *difficulty of a chemical nature*, and we strongly recommend physicists to make known in their works the method of preparation, and if possible the results of the analysis of the different bismuths which they have examined.

By means of the two following Tables my results may be easily compared with those of other physicists. It appears by the results that the bismuths studied by Messrs. L. Lorenz and Righi were pure.

Bismuths.	Slowly cooled.		Tempered.		Compressed.	
	R_0.	K *.	R_0.	K.	R_0.	K.
Brommsdorff...	$10^3 \times 109{\cdot}90$	0·00325	$10^3 \times 139{\cdot}86$	0·00199		
Classen I.	......	0·00076	$10^3 \times 246{\cdot}91$	−0·000603		
Classen II. *a* ...	$10^3 \times 124{\cdot}69$	0·00299	$10^3 \times 166{\cdot}66$	+0·00106	$10^3 \times 236{\cdot}96$	−0·00049
II. *b* ...	......	......	$10^3 \times 157{\cdot}48$	+0·00128	$10^3 \times 251{\cdot}26$	−0·00083
II. *c* ...	......	......	$10^3 \times 163{\cdot}40$	+0·00116	$10^3 \times 268{\cdot}10$	−0·00105
Classen III. *a*...	$10^3 \times 156{\cdot}74$	0·00132	$10^3 \times 204{\cdot}50$	+0·00009		
III. *b*...	......	0·00101	$10^3 \times 208{\cdot}33$	+0·00005		
Classen IV. *a*...	$10^3 \times 170{\cdot}07$	0·00126	$10^3 \times 207{\cdot}47$	−0·00004		
IV. *b*...	$10^3 \times 168{\cdot}35$	0·00113	$10^3 \times 212{\cdot}94$	−0·00008		
Electrolyzed Bi.	$10^3 \times 107{\cdot}99$	0·00429	$10^3 \times 108{\cdot}69$	+0·00422		

* It is to be observed that all the coefficients of K in this column are *positive*.

Observations.	Experimenter.	R_0.	K.
	Lenz.	$10^3 \times 82{\cdot}36$	
	Matthiessen.	$10^3 \times 131{\cdot}50$	
	Matthiessen.	$10^3 \times 125{\cdot}70$	+0·00418
	F. Weber.	$10^3 \times 119{\cdot}33$	
	L. Lorenz.	$10^3 \times 107{\cdot}64$	+0·00475
Bi cooled slowly.	Leduc.		+0·00455
Bismuth plate.	Leduc.		−0·00127
	Righi.	$10^3 \times 108{\cdot}49$	
Bismuth plate.	Von Ettingsh. & Nernst.	$10^3 \times 208{\cdot}33$	−0·0012
	C. L. Weber.		−0·0006
	Lenard & Howard.		+0·0052
Bi cooled slowly.	Edw. van Aubel.	$10^3 \times 107{\cdot}99$	+0·00429
Bi hardened.	Edw. van Aubel.	$10^3 \times 108{\cdot}69$	+0·00422

I have great pleasure in expressing my thanks to Profs. Wüllner and Classen for having so kindly furnished me with the valuable materials necessary for my researches.

Physical Laboratory of the Polytechnic School,
Aix-la-Chapelle.

PROCEEDINGS

OF

THE PHYSICAL SOCIETY

OF LONDON.

November 1889.

XXXIII. *On the Clark Cell as a Source of small Constant Currents.* *By* Richard Threlfall, *M.A.*, *Professor of Physics, University of Sydney*, *and* Arthur Pollock, *B.Sc.*[*]

[Plates VI. & VII.]

There are several methods by which the potential-difference (for the future written P.D.) of the plates of a voltaic cell may be measured. To measure the E.M.F. it is, as a rule, necessary to prevent the cell under examination from yielding any appreciable current while the estimation of its terminal P.D. is being made.

The electrometer is consequently the most obvious instrument to use in such an investigation; but, as a matter of fact, it is infinitely inferior both in convenience and accuracy to the galvanometer.

All the experiments about to be described were made with a galvanometer in the manner adopted by Lord Rayleigh and Mrs. Sedgwick (Phil. Trans. ii. 1884, "On the Electrochemical Equivalent of Silver, and on the Absolute Electromotive

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[*] Read March 23, 1889.

Force of Clark Cells"). The method admits of an estimation of the terminal P.D. of a cell, either at rest or when (by the use of separate sets of resistance-coils to short-circuit its terminals) it is yielding a current of any desired value.

Let E be the E.M.F. of the cell, P.D. its terminal potential-difference, C the current passing through it, R its internal resistance, R′ the external resistance short-circuiting its terminal, Δ the observed change of P.D. at any instant when the current is flowing, measured from the value of P.D. when the cell is at rest. Then

$$\begin{aligned}\Delta &= \text{P.D. when cell is at rest} - \text{P.D. when cell is giving current C},\\ &= E-(E-CR),\\ &= CR \text{ when there is no polarization.}\end{aligned}$$

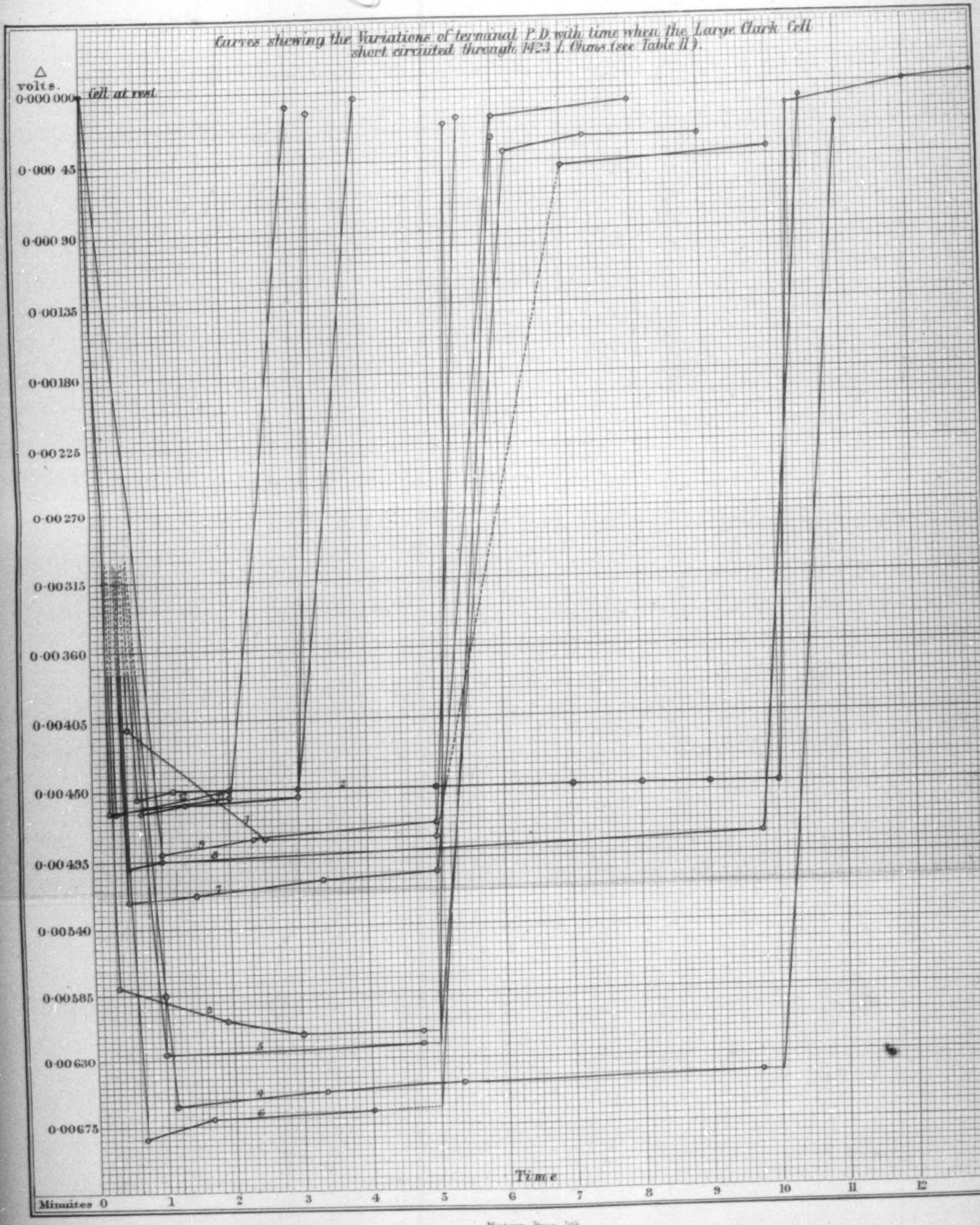
Now at any instant let the E.M.F. change by polarization so that it becomes E′, and let the internal resistance become R″. Then, on short-circuiting,

$$\begin{aligned}\Delta &= E-(E'-C'R''),\\ &= E-\frac{E'R''}{R'+R''}.\end{aligned}$$

Consequently, if E′ diminishes or R″ increases, we shall have increased values of Δ. These increased values will then indicate what is generally known as polarization. It is possible, however, that there may be another sort of polarization (if we may venture to so extend the term) of which an increasing E′ or diminishing R″ is characteristic. If such an action exists and tends to increase as time goes on, we shall observe a continual decrease of Δ as time progresses after short-circuiting.

Such an effect has been pretty constantly observed; and as it was not due to any change in the compensator, we are obliged to attribute it to a small variation in the Clark cell itself in one or both of the directions mentioned.

On referring to Wiedemann (*Die Lehre von der Electricität*, edition 1883, vol. ii. p. 708) we found a statement, on the authority of Hockin and Taylor (Journ. Soc. Teleg. Eng. viii. p. 282, to which we have not access), to the effect that an increase of thickness of a polarizing layer of hydrogen on mer-



Curves shewing the Variations of terminal P.D. with time when the Large Clark Cell short circuited through 1423 I. Ohms (see Table II).

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cury causes the polarized surface to become more positive. The method employed, and the statement that finally bubbles of gas may be given off by sufficiently diminishing the area of the mercury surface, suggests that the polarizations used in the experiments were very strong. The electrolyte was dilute sulphuric acid.

Some experiments were made by us, using a similar but improved method and with zinc-sulphate solution as an electrolyte. The sort of effect we were in search of was an increase of potential on one mercury surface of about 0·0002 volt. Every precaution was taken in securing mercury and sulphate of zinc identical with those used in Clark cells, and in checking the behaviour of the electrodes. An effect larger than that we were in search of could generally be detected on altering the extent of the mercury surfaces, but we finally convinced ourselves that the capillary assumptions made were of such a nature as to require revision. This would in itself have involved extensive research, and we did not feel that the end was likely to prove in any way commensurate with the labour of reaching it; consequently we are unable to say whether the potential of the mercury surface does or does not rise with increased density of hydrogen deposition. In any case the small deviation of the value of Δ observed from its value, as accounted for by the cell resistance, shows that very little (if any) hydrogen really reaches the mercury surface. On the other hand, it is easy to see several reasons why R'' might become smaller sufficiently to account for the effect observed. It may be worth noting that the possibility of collecting evidence as to the existence or nonexistence of polarization when the change of Δ with flux of time is very small becomes a question of indirect evidence.

We did not habitually estimate changes of Δ of less magnitude than 0·000045 volt, and consequently what follows refers only to changes of greater amount than this. By our method the obvious test for polarization would be a measurement of the resistance of the cell at the actual moment when its terminal P.D. was obtained. This is experimentally impossible (as we consider) with the accuracy necessary, to say nothing of the continual changes in the value of the current taken from the cell which are to be expected during the adjustment of the bridge. The tests which we apply can be rendered evident most easily by means of a diagram. Consider first the case of no polarization, *i. e.* no change of E or R. If we plot values of Δ as ordinates when C is nearly constant, and is assumed to be exactly so, and times reckoned from the epoch of short-circuiting as abscissæ, then the resulting curve will be three sides of a rectangle if it is prolonged till the moment of reinsulating the plates of the cell. This in fact corresponds simply to the case of $\Delta = CR$. Now let polarization set in. The curve giving the value of Δ will rise or fall, and the amount of rise or fall will be a sort of measure of the polarization.

Again, let currents of different values be taken from the cell and observe whether $\Delta = KC$, where K is a constant (it is of course assumed that R cannot be got with sufficient accuracy by other means, and the determining of it in this way would beg the question). We shall show that when certain conditions are complied with, this is nearly but not quite true. Consequently to the high degree of accuracy which has been aimed at in our experiments, it is never true to say that the Clark cell remains unpolarized when changes of K are considered as evidences of polarization. In order to exhibit the changes actually occurring in K, the following Table has been prepared. To get the value of C, corresponding to any observed value of Δ, the experimental P.D. was taken and divided by the external resistance. The table shows what the cell variations actually are—expressed as if they were all to be traced to actual changes of resistance. It will be noticed that the values of K increase as the current diminishes, showing that what is ordinarily called polarization is less with greater currents than with small. It is fair, however, to state that the variations of P.D. in the latter case were nearer to our experimental limit of discrimination, and consequently are to be treated as having less weight than those referring to greater currents.

Table I.

Table giving the value of K from the equation $\Delta = KC$.

Date.	External resistance.	Δ.	Terminal P.D.	Current.	K.	
1888.	L. ohms.	volts.	volts.	amperes.		
July 26	143	0·04730	1·38776	0·009704	4·874	
26	143	0·04590	1·38911	0·009714	4·725	
26	143	0·04545	1·38956	0·009717	4·677	
26	143	0·04606	1·38913	0·009713	4·741	
26	494	0·01593	1·41917	0·002872	5·545	
26	987	0·00729	1·42781	0·001446	5·039	
	1423	0·005217	1·430663	0·001005	5·189	Mean of 13 obs. during July & Aug.
July 24	2862	0·00254	1·43323	0·0005007	5·072	Mean of 2 obs.
24	4293	0·00180	1·43383	0·0003339	5·384	
Aug. 21	10000	0·00076	1·43538	0·0001435	5·295	Mean of 2 obs.
Oct. 22	10000	0·00079	1·43448	0·0001434	5·507	Mean of 4 obs.
July 31	20000	0·00058	1·43489	0·00007174	8·084	
31	30000	0·00045	1·43507	0·00004783	9·407	
31	40000	0·00036	1·43521	0·00003588	10·03	
31	50000	0·00027	1·43530	0·00002870	9·406	
31	60000	0·00023	1·43539	0·00002392	9·614	
31	70000	0·00023	1·43544	0·00002050	11·21	
31	90000	0·00018	1·43554	0·00001595	11·28	
Nov. 1	10000	0·000630	1·42837	0·00014284	4·415	Temp. 21°·6 C. Mean of 2 obs.
2	10000	0·000765	1·42853	0·00014285	5·355	Temp. 21°·7 C.
1	10000	0·000630	1·42107	0·00014211	4·433	Temp. 34°·2 C.
1	10000	0·000630	1·42107	0·00014211	4·433	Temp. 33°·9 C.
1	10000	0·000630	1·42117	0·00014211	4·433	Temp. 33°·0 C.
1	10000	0·000720	1·42158	0·00014216	5·064	Temp. 32°·0 C.
1	1450	0·00293	1·41997	0·00097931	2·991	Temp. 30°·9 C.
1	1450	0·00257	1·41913	0·00097868	2·626	Temp. 30°·6 C.
2	1450	0·00315	1·42605	0·00098351	3·203	Temp. 21°·7 C.

It is, however, also true that polarization is never sufficient to cause an uncertainty of 0·5 per cent. in the value of a current of nominally ·001 ampere yielded by the kind of cell about to be described.

The greatest observed uncertainty during three months of continued testing and varying of conditions was 0·6 per cent.; of this probably only half at most was to be traced to polarization.

On an average the uncertainty did not exceed 0·1 per cent.; so that we consider that in stating that the Clark cell may be used with security to give currents of a value not differing by more than 1 per cent. from their nominal value we shall leave a really unnecessarily large margin for uncertainty in the actual value of a properly constructed external resistance.

In May 1888 we began to investigate the behaviour of the

Clark cell when giving small currents, with a view to applying it to the calibration at any time of a certain galvanometer which we had designed for one of the hospitals of Sydney. We were aware that the Clark cell had the reputation of being easily polarized, but this seemed to us probably to arise from the fact that the cells as usually made expose very small surfaces of zinc and mercury. We therefore constructed some cells of considerable size in order to find out whether our idea was or was not correct. The cells constructed and tested were the following:—

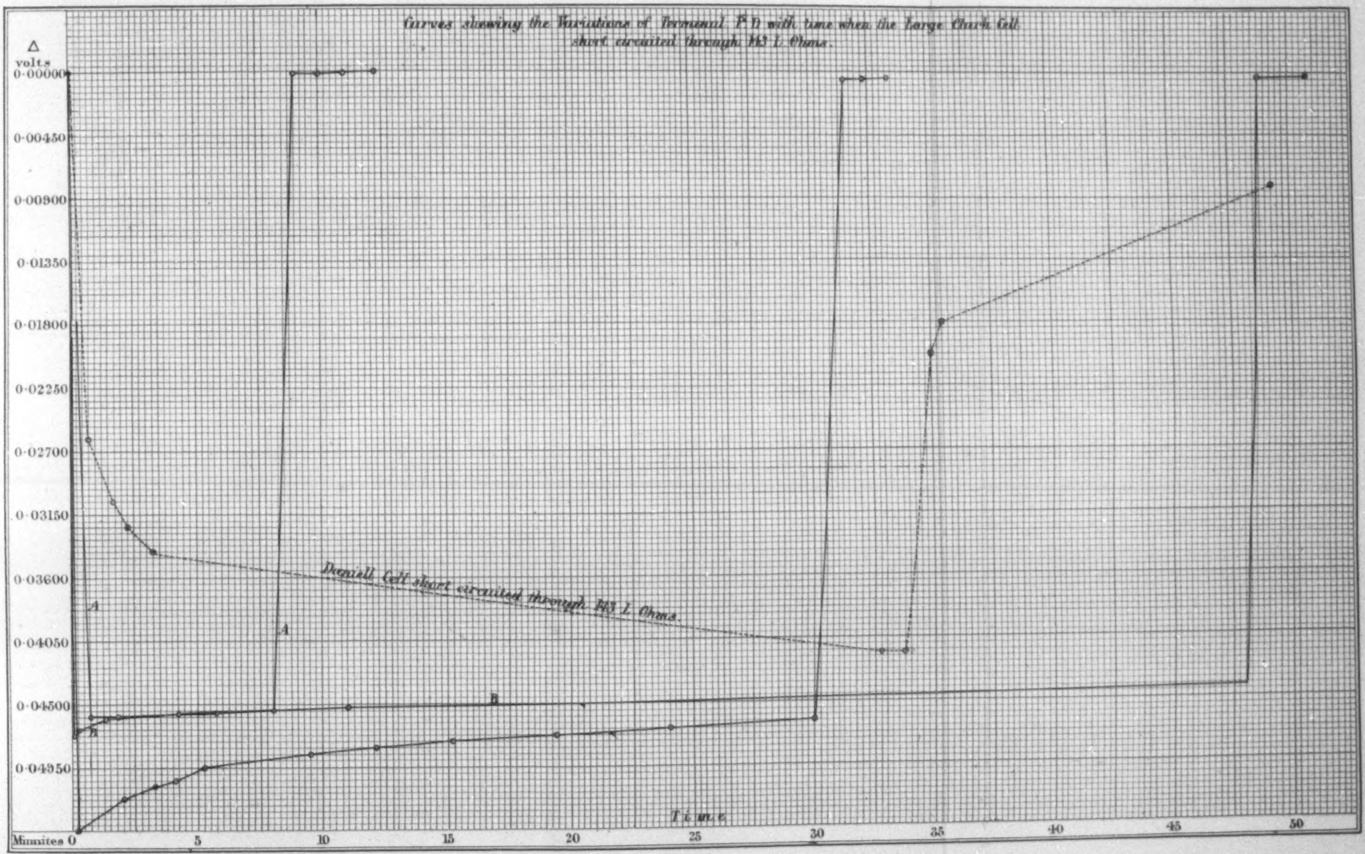
(1) A set of 40 test-tube cells of the ordinary pattern. The tube of each cell was three inches long and half an inch in internal diameter. The area of surface of mercury exposed was about 0·2 square inch or 1·3 square centimetre. The cells differed considerably from one another as regards resistance, the average resistance being about 40 l. ohms. The positive electrodes were bits of "pure" zinc rod as supplied by the dealers, ¼ inch in diameter, but of rather variable lengths. The areas of the zinc surfaces immersed differed considerably in the different cells. These cells were first set up in June 1886, and have been tested at intervals ever since. Their behaviour is quite satisfactory with the exception of that of No. 18, which was out by about ·001 volt from the beginning.

(2) Thirteen cells, similar to the above, made up in 1888 for the purpose of this investigation, and charged with a sample of the same mercury as used by Lord Rayleigh, *i.e.* taken originally from the same bottle. Our object in preparing these cells was to connect our work directly with Lord Rayleigh's measurements.

Two days after being set up, the mean E.M.F. of these 13 cells exceeded the mean E.M.F. of the 40 cells by 0·00045 volt.

Considering that the paste had not been neutralized, the agreement is closer than might have been expected.

(3) A cell, generally called the "master cell," made up in a glass cylinder about 12 inches high and 1¾ inch in diameter. The zinc plate, which was just small enough to pass down the cylinder, was cut from a sheet of ordinary commercial zinc, and had been amalgamated before being placed in the cell. The mercury surface was about three square inches in area. This cell was generally used to oppose the others.



Curves shewing the Variations of Terminal P.D. with time when the Large Clark Cell short circuited through 143 L Ohms.

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(4) Two cells set up in glass jars similar to the above, but shorter and of greater diameter. Each of these cells exposed a mercury surface of about 12 square inches, and a zinc surface of nearly equal amount. As it was expected that considerable solution of zinc would occur in these cells, the plates were wrapped up in parchment paper. One of the cells was called the "large cell," and the other the "hospital cell," as it was actually in tentative use in the hospital during the progress of the testing, and was occasionally brought over by hand for comparison.

The investigation of the peculiarities of these cells was directed to the elucidation of the following points:—

1. The discovery of the most probable value of the E.M.F. of the cell at rest with a view to finding its terminal P.D. when yielding a given current.

2. The effect of the extent of mercury and of zinc surface in determining the maximum current which can be taken from any cell with a given allowable amount of uncertainty.

3. Does the value of Δ change with lapse of time reckoned from the epoch of short-circuiting? and if so what is the best time for making an observation?

4. Does the value of Δ change appreciably from day to day, the external resistance through which the cells are short-circuited remaining constant, or generally has the immediate previous history of the cell anything to do with the value of Δ in short-circuiting?

5. Has change of temperature any considerable effect on the value of Δ, all the other conditions remaining constant?

6. The reliability of the large cell to give ·001 ampere through a platinoid resistance assumed correct at 15° C.

Method of Experimenting.—As already stated, the method described by Lord Rayleigh (*loc. cit.*) has been used throughout. All the measurements were made in a room with a southern aspect, and therefore not subject to any sudden change of temperature. Light was admitted through a sheet of paper forming a window in otherwise closed shutters. The temperature lay between 14° and 16° C. during the winter months, in which the experiments were made. At the close advantage was taken of a hot day to repeat the investigation at 23° C.

The galvanometer generally used had a resistance of about six thousand ohms, and a sensitiveness of about four scale-divisions for 10^{-11} amperes by direct test. This was sometimes supplemented by another galvanometer of nearly sixteen thousand ohms resistance, and whose sensitiveness could be brought up to several scale-divisions for 10^{-13} amperes.

The smallest change of P.D. usually noted was that compensated by altering the resistance in the compensator by 0·1 ohm corresponding to 0·000045 volt.

In addition to the apparatus requisite for finding the terminal P.D. of any cell at rest, resistances were required for the purpose of short-circuiting the terminals of the cell. For this purpose a very large box of German-silver resistance-coils was used. All the coils had been previously compared with standards by Carey Foster's method, and were in fact adjusted by this method in the laboratory. The corrections are in no case large enough to require taking into account. The insulation of the various portions of the apparatus was carefully attended to by the use of blocks of paraffin and was very necessary.

For the determination of the most probable value of the E.M.F. of any cell, comparisons of all the cells were made by the method mentioned, and the mean value was taken as identical in absolute measure with Lord Rayleigh's mean value, *i. e.* 1·43500 Rayleigh volt at 15° C. Many complete comparisons were made during three months, partly to find the mean and partly to check the action of each cell. The large cell and hospital cell were compared with the small ones, sometimes directly and sometimes through the intermediary of the master cell. The changes of temperature occurring were too small to affect the difference between the cells within our limits of observation. One hundred and sixty comparisons were used in computing the probable values of the E.M.F. of the large cell and of the hospital cell at rest.

Nothing could have been more satisfactory than the behaviour of all the cells, though they were badly used between whiles. Suffice it to say that the result was almost the same whatever cell was used to oppose the others; and throughout the months of June, July, and August the mean E.M.F. of the large cell has the value of 1·43588 Rayleigh volt at 15° C., and the hospital cell the corresponding value of 1·434608 volt under the same conditions.

The second point referring to the effect of variable zinc and mercury surface on the observed values of Δ with varying currents was practically the first one to be investigated. Without going into the large amount of numerical information collected, we may state the following results:—

When the change of Δ during five minutes after short-circuiting is less than or equal to about $\frac{1}{10}$ per cent. of its value immediately after short-circuiting, the currents stand in about the order of the extent of mercury surface exposed. From a study of the small cells amongst themselves, it was found impossible to give any satisfactory explanation (of an experimental character) of the large difference of the maximum possible currents (*i. e.* those leading to a change of P.D. less than $\frac{1}{10}$ per cent.) that were observed.

The mercury surface was practically the same in all these cells, but the zinc surface varied. The currents did not stand at all in the order of the zinc surfaces. The most important factor appeared to be the thickness of the layer of mercurous sulphate, but it was not the only one. If the layer was less than about a quarter of an inch thick, the cells were bad. The result of it all was that the most important thing besides having plenty of mercurous sulphate is to have large surfaces of mercury and possibly of zinc. For this reason the large cells which it was intended to use in practice received the greatest share of attention, and we shall confine ourselves to describing the phenomena exhibited by them.

The results of some typical measurements are given in the following tables and curves, which will be found to contain information as to the effect of previous history as well. It may be stated that it was found possible to estimate the value of Δ within less than half a minute after short-circuiting.

From our point of view, viz. the applicability of the cell as a source of current of 0·001 ampere, it is immaterial whether there is polarization or not so long as it is not of a creeping character—nor then if the rate of creep is constant.

The answer to the third question is from the practical point of view absolutely vital and has received most attention.

Table II. gives the experience we have had. As a result the time finally chosen at which to make an observation is two minutes after short-circuiting. The information contained in the Table is also given graphically in Plate VI., where curves are drawn with values of Δ as ordinates and time as abscissæ.

In Plate VII. corresponding curves are drawn when the cell is yielding a current of nearly 0·01 ampere. They are drawn from observations taken on July 26 with a resistance short-circuiting the terminals of the large cell of 143 l. ohms. (See Table I.)

TABLE II.

Variations of terminal P.D. with time when large Clark cell is short-circuited through 1423 l. ohms.

Δ is the terminal P.D. immediately before changing the resistance between the poles from ∞ to 1423 l.o. minus the terminal P.D. at the times given in the 2nd column.

Date.	Time.	Δ.	Resistance between Poles.
1888.	h m s	volts.	
July 24.	3 30 0	0·000000	Changed from ∞ to 1423 l. ohms.
	30 20	+0·004095	1423
(1.)	32 0	+0·004815 steady at this.	1423
	35 0	....	Changed to ∞.
	37 0	+0·000595	∞
	40 0	+0·000405	∞
	45 0	+0·000360	∞
	51 0	+0·000360	∞
July 24.	3 57 0	0·000000	Changed from ∞ to 1423 l. o.
	57 20	+0·004635	1423
(2.)	59 0	+0·004500	1423
	4 2 0	+0·004500	1423
	4 0	+0·004500	1423
	5 0	+0·004500	1423
	6 0	+0·004500	1423
	7 0	....	Changed to ∞.
	7 20	+0·000135	∞
	9 0	0·000000	∞
	10 0	−0·000045	∞
	12 0	−0·000090	∞
	15 0	−0·000135	∞
	23 0	−0·000135	∞
July 26.	3 7 0	0·000000	Changed from ∞ to 1423 l. o.
	7 20	+0·005805	1423
(3.)	8 55	+0·006030	1423
	10 0	+0·006120	1423
	11 45	+0·006120	1423
	12 0	....	Changed to ∞.
	13 10	+0·000405	∞
	14 20	+0·000315	∞
	16 0	+0·000315	∞

Table II. (*continued*).

Date.	Time.	Δ.	Resistance between Poles.
1888.	h m s	volts.	
Aug. 8.	3 17 0	0·000000	Changed from ∞ to 1423 l. o.
	18 0	+0·006588	1423
(4.)	20 20	+0·006499	1423
	22 20	+0·006454	1423
	25 45	+0·006409	1423
	26 0	....	Changed to ∞.
	27 0	+0·000269	∞
Aug. 8.	3 30 0	0·000000	Changed from ∞ to 1423 l. o.
	31 0	+0·006230	1423
(5.)	34 45	+0·006185	1423
	35 0	....	Changed to ∞.
	35 20	+0·000226	∞
Aug. 9.	12 57 0	0·000000	Changed from ∞ to 1423 l. o.
	57 40	+0·006819	1423
(6.)	58 40	+0·006685	1423
	1 1 0	+0·006640	1423
	2 0	....	Changed to ∞.
	3 0	+0·000315	∞
Aug. 20.	5 17 0	0·000000	Changed from ∞ to 1423 l. o.
	17 30	+0·005227	1423
(7.)	18 30	+0·005182	1423
	20 20	+0·005114	1423
	22 2	+0·005047	1423
	22 10	....	Changed to ∞.
	23 0	+0·000180	∞
	25 0	+0·000090	∞
Aug. 20.	5 25 0	0·000000	Changed from ∞ to 1423 l. o.
	25 30	+0·005024	1423
(8.)	26 0	+0·004979	1423
	34 45	+0·004822	1423
	35 0	....	Changed to ∞.
	35 30	+0·000090	∞
Aug. 21.	4 5 0	0·000000	Changed from ∞ to 1423 l. o.
	6 0	+0·004906	1423
(9.)	7 20	+0·004821	1423
	10 0	+0·004726	1423
	10 0	....	Changed to ∞.
	10 30	+0·000180	∞

Table II. (*continued*).

Date.	Time.	Δ.	Resistance between Poles.
1888.	h m s	volts.	
Aug. 21.	4 12 0	0·000000	Changed from ∞ to 1423 l. o.
	12 45	+0·004636	1423
(10.)	13 20	+0·004591	1423
	15 0	+0·004551	1423
	15 0	....	Changed to ∞.
	16 0	+0·000045	∞
Aug. 21.	4 19 0	0·000000	Changed from ∞ to 1423 l. o.
	19 15	+0·004641	1423
(11.)	21 0	+0·004551	1423
	21 0	....	Changed to ∞.
	22 0	+0·000090	∞
Aug. 21.	4 25 0	0·000000	Changed from ∞ to 1423 l. o.
	25 40	+0·004546	1423
(12.)	26 10	+0·004505	1423
	28 0	+0·004505	1423
	28 0	... .	Changed to ∞.
	28 20	+0·000135	∞
Aug. 21.	4 30 0	0·000000	Changed from ∞ to 1 423 l. o.
	31 0	+0·004551	1423
(13.)	36 0	+0·004551	1423
	36 0	....	Changed to ∞.
	37 0	+0·000135	∞

The scale of the curves is very big in order to bring out differences of observed values of Δ; it will be noticed, however, that Δ was in all cases extremely small.

The general effect is seen to be a decrease of Δ as time goes on from the epoch of short-circuiting. This has already been discussed. For comparison a large Daniell cell was examined at the same time, and the dotted curve in Plate VII. referring to it will show its unexpected inferiority. The cell exposed about 80 square inches of copper, and was allowed to work long enough to obtain a clean electrolytic surface; zinc sulphate solution was used in the inner cell. The resistance of the cell was very low, and the curve taken is for its behaviour when short-circuited through 143 l. o. This gives a current of about 0·0067 ampere; and a current-density of 0·000012 ampere per square centimetre of copper. In the large Clark cell the curve for a current of about 0·01 ampere (condemning the cell for so great a current) corresponds to a current-density of 0·00012 per square centimetre of mercury surface. The more nearly corresponding curves, taking current-density as the criterion of correspondence, are to be found by using one of the Clark-cell curves for 0·001 ampere. In this case the latter is infinitely superior, and is even superior when giving 0·01 ampere with ten times the current-density. The trouble and difficulty involved in setting up a Daniell cell would quite independently render it useless for our purpose, so the comparison was on all accounts pushed no further.

It will be noticed that the greatest fall of terminal P.D. was observed on August 9th and amounted to 0·006685 volt, being 0·001468 greater than the mean. Now, on July 26th occurred the smallest observed value of the cell at rest, this was 1·435415 volt. Suppose that in practice these causes of error (here observed on different days) conspired, we should have taken a real P.D. of 1·428730 volt for 1·430663 volt. The error thus introduced would have been ·002 volt nearly, or an error of about 0·14 per cent.

Similarly taking the greatest observed terminal P.D. at rest and the smallest fall, the error the other way if these causes conspire amounts to calling a real P.D. of 1·43186 only 1·430663, or about 0·08 per cent.

It remains to discuss the effect to be expected from changes of temperature. The temperature-coefficient of the Clark cell is of course known, but we have as yet given no evidence as to any possible temperature-change of Δ other than what is to be expected from the known change in the cell itself. To test this matter, on November 1st the large cell was placed in a very large vessel filled with water at a temperature of about 38° C., and left there for a considerable time. The bath was well wrapped up, and during 37 minutes the temperature fell to 34°·2 C. when tested by a standard thermometer. The fall of temperature being so slow allows us to hope that the temperature of the cell during the experiments did not differ to any considerable extent from that of the water. Moreover the E.M.F. of the cell had again become constant and had fallen through 0·00729 volt. By comparison with the 40 cells, on October 20th, it was found that the E.M.F. of the master cell was 1·43356 volt at 17° C. By a comparison of the large cell with the master cell at the time of the experiment and at a temperature of 21°·5 C., its E.M.F. was found to be 1·42900 volt; consequently at 34°·2 C. the E.M.F. was 1·42171 volt. The results are given in Table III.

TABLE III.

Fall of terminal P.D. of Large Cell when short-circuited through 10,000 l. ohms.

Date.	Temperature.	Δ.
1888.		volt.
November 1	21°·6 C.	0·000675
„ 1	21·6 C.	0·000585
„ 2	21·7 C.	0·000765
„ 1	34·2 C.	0·000630
„ 1	33·9 C.	0·000630
„ 1	33·0 C.	0·000630
„ 1	32·0 C.	0·000720

Fall of terminal P.D. of Large Cell when short-circuited through 1423 l. ohms.

Date.	Temperature.	Δ.
1888.		volt.
November 1	30°·9 C.	0·002925
„ 1	30·6 C.	0·002565
„ 2	21·7 C.	0·003150

The result to be noted is that the value of Δ may be said to have changed in the practical case from 0·005 to 0·003 volt between 15° and 30° C.—a very small variation considering the complexity of the changes which might have taken place; consequently we need not fear that any ordinary change of temperature will introduce any appreciable error except that arising from the ordinary temperature-coefficient of the cell.

This experiment gives incidentally the value of the temperature-coefficient for rapid heating over the range comprised between 21°·7 and 30°·9 C. Assuming Lord Rayleigh's temperature-coefficient for the range of temperature between 15° and 21°·7 C., *i. e.* for temperatures which had been acquired so slowly as to allow the zinc-sulphate solution to reach its proper concentration with complete certainty, we have the following:—

Lord Rayleigh's formula is

$$E = 1{\cdot}435 \left[1 - 0{\cdot}00077\,(t - 15^\circ)\right],$$

whence it follows that (calling the mean E.M.F. of the 40 cells at 15° C. = 1·43500 volt) at 17° C., the temperature of comparison, the mean E.M.F of the 40 cells is 1·43279 volt; and hence the E.M.F. of the master cell, as it was 0·00077 volt greater than the mean of the 40 cells, = 1·43356 volt. At 21°·5 C. the E.M.F. of the master cell (using Lord Rayleigh's coefficient) = 1·42859 volt.

At the time of the experiments on the large cell the temperature of the laboratory was 21°·5 C. and E.M.F. large cell − E.M.F master cell = + 0·000405 volt; therefore E.M.F. of large cell = 1·428995 volt at a temperature of 21°·5 C. Thirty-seven minutes after having been put in the bath the E.M.F. of the large cell was steady at 34°·2 C. and E.M.F. large cell − E.M.F. master cell = − 0·006885 volt, the master cell being still at 21°·5 C. Hence the fall of E.M.F. of the large cell for a rise of 12°·7 C. was 0·00729 volt, and its actual E.M.F. was 1·4217 volt.

Hence taking our standard temperature at 21°·5 C., and using Lord Rayleigh's formula with the constant appropriate to 21°·5 C., we have

$$1{\cdot}4217 = 1{\cdot}42900\,\{1 - K \times 12{\cdot}7\},$$

whence K = 0·000402 between 21° and 34° C. say, and for rapid heating.

There is no reason why this value, deduced from a somewhat rapid change of temperature, should agree with the value obtained by Lord Rayleigh in cases where the temperature varied much more slowly and was considerably lower. We have evidence from the history of the 40 cells to the effect that at all events up to 24° C. the *differences* of E.M.F.

are practically the same as they are at 15° C. In the case to which these results have been applied the external resistance through which the cell has to work is composed partly of a copper and partly of a platinoid resistance-coil. The copper coil in this case belongs to the galvanometer and the error thereby introduced, though well within the limits laid down for this particular application, is much greater than it would have been had the galvanometer-winding consisted of German-silver or platinoid wire. There would in this or similar cases have been no objection to the use of such a winding.

At 15° C., then, to get the most accurate current of 0·001 ampere from the large cell we ought to use an external resistance of 1430·7 l. ohms, if we ignore the difference between the Rayleigh volt and the Congress volt, as we may very well do.

It is now left for us to determine what error a change of temperature of 8° on either side of 15° C. is likely to cause. In this country, New South Wales, we have to work pretty often at temperatures as high as 23° C., and we imagine that no one would work below 7° C. if it could be avoided; consequently we take these as our limits.

We have already found that perturbations of the P.D. of the cell itself may introduce an error in that quantity to the extent say of 0·2 per cent. (the greatest we got by combining the most aberrant observations of different days was 0·14 per cent.).

On the strength of the experiment above 30° C. and the experience with the small cells from 14° to 23° C., we assume that no change in Δ will occur otherwise than that dependent on the ordinary coefficient. This is not strictly legitimate for lower temperatures, but it is difficult to obtain them much lower here.

We know that we shall have two causes at work tending to change our current value; these will be in the same direction.

The E.M.F. will tend to diminish with increased temperature, and the resistance of the wire will increase.

The cell-resistance change is of course included in variations of Δ, and these we have shown to be negligible for practical purposes when the external resistance is large.

By combining the effects of change of resistance and of

E.M.F. (using Lord Rayleigh's coefficient) we get a temperature-coefficient for the current yielded.

The temperature change of resistance of platinoid wire is about 0·022 per cent. per degree, and the change of E.M.F. when the cell rises or falls 8 degrees from 15° C. is about 0·0088 volt. Consequently the error arising from this cause is about 0·6 per cent. Adding all the possible causes of error together we find that the uncertainty *may* arise to 0·96 per cent., say 1 per cent., in the value of the current. It need hardly be added that of these causes of error the two latter are easily allowed for where accuracy is required, and the former is very small. We do not think that in such a case an error of more than 0·1 per cent. ought to be expected.

For ordinary purposes for which the current may be used an accuracy to within 1 per cent. is probably all that will be required.

We have to thank Lord Rayleigh and Prof. Ayrton for some valuable suggestions as to the mode of statement of the above results.

XXXIV. *On the Application of the Clark Cell to the Construction of a Standard Galvanometer.* *By* Professor RICHARD THRELFALL, *M.A.*[*]

[Plate VIII. figs. 1 & 2.]

IN the instrument which forms the subject of this paper the experience obtained with Clark's cell is utilized to obtain a simple means of standardizing a working current-measurer. The construction of the instrument itself will be readily understood from the accompanying figures. Its chief features are:—

(1) The arrangements which have been made for the support of the controlling magnet and for its adjustment: this latter can be readily carried out without disturbing the suspension.

(2) The damping of the needle by means of a thin copper cylinder attached to a bit of fine wire and dipping in clove-oil.

(3) The mechanical arrangements of this part of the appa-

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[*] Read March 23, 1889.

ratus, allowing of the easy suspending of the mirror and adjustment of the cylinder in the oil.

(4) The arrangements for the testing of the galvanometer by means of the Clark cell.

(5) The curving of the scale, so as to obtain direct tangent-readings from a scale of equal parts.

The single coil of the instrument consisted of a rectangular section of winding of 200 turns of No. 22 B.W.G. copper wire. Resistance 2·02 ohms at 16° C.

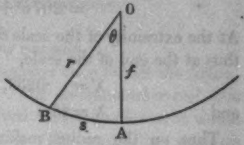
This coil had an axial dimension of 1·3 centim., a radial dimension of 1·3 centim., and the radius of the inner layer of winding was 3·7 centim. The coil was supported so as to be capable of sliding backwards and forwards with respect to the suspended parts, keeping parallel with itself to a considerable degree of accuracy. The slide was of carefully crossed wood, the moving portion being kept in its position in the grooves by means of half carriage-springs at each end. A somewhat similar arrangement is adopted in the sliding wooden parts of the Kew magnetometer.

There are three marked positions of the coil with respect to the suspended needles.

Though the mirror is rather large the magnets are small, in the ordinary sense, *i. e.* about a quarter of an inch long. A calculation was made by Mr. Adair on the law of deflexion of the magnet by the coil at the three distances and up to deflexions of about 15°. The method adopted in this calculation was the expansion in 'Spherical Harmonics' used by Maxwell, part 4, chap. xiv. The tangent law was found to be practically true, *i. e.* the deviation from it would never introduce an error of more than about ·5 per cent. The error was greater the less the deflexion, and was negligible for the accuracy required, which was of course not very great. The error arising from partly neglecting the torsion of the silk fibre was also investigated and found to be without influence: the fibre was seven inches long.

The divided scale was one of Elliott's scales, in which 360 divisions correspond to 229 millimetres. The distance from the mirror to the scale was 1095·7 scale-divisions, or about seven hundred millimetres. The problem of finding the form of the curve into which it is necessary to bend the scale of equal parts so as to read direct currents was solved by Mr. Adair; as we could find no previous record of this solution, I will give it here.

Let A B be a portion of the curve required; let O A$=f$ the apsidal distance, θ the polar angle subtended by A B, OB $=r$ the distance of the light-spot from the mirror. The incident light falls along A O. A B$=s$.



The form of the curve, assuming that the galvanometer obeys the tangent law, is

$$s=2f\tan\frac{\theta}{2},$$

or

$$\frac{ds}{d\theta}=f\sec^2\frac{\theta}{2};$$

and the differential equation giving r in terms of θ is

$$\left(\frac{dr}{d\theta}\right)^2+r^2=f^2\sec^4\frac{\theta}{2}.$$

This is insoluble in general terms: but if the range of θ is small we can develop in powers of θ, and assume $r=f+\lambda$, where λ is the addition to the radius of the circle whose centre is O and radius f. Thus the differential equation for λ becomes, by retaining terms in θ^4,

$$\frac{1}{f^2}\left(\frac{d\lambda}{d}\right)^2=\left[2\left(\frac{\theta}{2}\right)^2+\frac{7}{3}\left(\frac{\theta}{2}\right)^4\right]-2\frac{\lambda}{f}.$$

Writing λ' for $\frac{\lambda}{f}$, we get

$$\frac{d\lambda'}{d\theta}+\left[\frac{\sqrt{2}}{\theta}-\frac{7\sqrt{2}}{48}\theta\right]\lambda'=\frac{1}{\sqrt{2}}\left[\theta+\frac{7}{48}\theta^3\right];$$

hence

$$\lambda'=\theta^{-\sqrt{2}}e^{\frac{7\sqrt{2}}{96}\theta^2}\left[c+\int\theta^{\sqrt{2}}\left\{1-\frac{7\sqrt{2}}{96}\theta^2\right\}\frac{1}{\sqrt{2}}\left\{\theta+\frac{7}{48}\theta^3\right\}d\theta\right]$$

Now $\lambda'=0$ when $\theta=0$; therefore the arbitrary constant $c=0$;

and to the order of θ retained we get

$$\lambda' = \frac{\sqrt{2}-1}{2}\theta^2 + \frac{4-\sqrt{2}}{96}\theta^4$$
$$= \cdot 207\,\theta^2 + \cdot 0269\,\theta^4.$$

At the extremity of the scale θ is about $\frac{1}{3}$ in circular measure; thus at the end of the scale,

$$\lambda' = \cdot 0233,$$

and $$\lambda = 25\cdot 5 \quad \text{scale-divisions.}$$

Thus on the radius making the angle $\theta = 19° 6'$, whose circular measure is $\frac{1}{3}$, the theoretical curve is outside the circle by a distance of 16 millim. Similarly, when $\theta = 10° = \cdot 174$ radius, $\lambda' = \cdot 006297$, and therefore $\lambda = 6\cdot 901$ scale-divisions $= 4\cdot 4$ millim.

With these numerical results the curve was laid out on millimetre paper, a template was cut to the curve, and the wooden back of the scale-holder was brought up to the template. The scale itself was carefully pinned to the wood at short intervals along its whole length.

The Clark cell supplied with the instrument had the same area of surface as the "large cell" referred to in our previous papers: it was tested from day to day alongside of the large cell, master cell, &c. The following particulars refer to this testing.

Internal resistance (August 28, 1888), roughly 5·5 ohms. August 29, at 16° C., hospital cell − master cell = −0·000852 volt. Terminal E.M.F. hospital cell, two minutes after short-circuiting through 1426 legal ohms, taking E.M.F. of master cell at 1·435:—

August 28.	1·4263	volt.
„ 29.	1·4261	„
„ 29.	1·4262	„

About ten minutes after short-circuiting, in each case the E.M.F. fell further ·0011 volt.

As the galvanometer was very dead-beat no calibration-experiment need take longer than 20 seconds, so that this is without effect.

In order to use the instrument the cell is coupled up in series with a platinoid resistance of 1417 legal ohms; the galvanometer-coil being itself 2·02 ohms, the cell about 5·5 ohms. Consequently the current used in testing is

$$c=\frac{1{\cdot}4262}{1417+2{\cdot}02}={\cdot}001007 \text{ ampere.}$$

This is quite near enough for our purpose.

In order to set up the galvanometer once for all the following dispositions were made. The galvanometer-coil was pushed up towards the suspended magnet as far as it would go; this was known as position O. The controlling magnets were then raised so as to give a mean sensitiveness; the known current was put on and reversed and the double deflexion noted. This deflexion was indicated by a fiducial mark on the scale. If at any time the sensitiveness of the instrument changes, it is only necessary to bring the coil up to its O position and raise or lower the controlling magnets by means of the adjusting arrangement till the deflexion reaches the fiducial mark.

When the coil is in the position A, ·001 ampere corresponds to 10 scale-divisions; and at B, to 1 scale-division. These points were found by using a very large storage cell, whose E.M.F. was tested whilst the calibration was going on, and employing suitable resistances from a thick wire box. The whole arrangement was tested by this means from ·001 to ·4 ampere, and it was found that the results were wonderfully consistent: this was possibly in part due to the mirror not being very good, and consequently the observation is so far wanting in accuracy. With the rather bad light-spot the readings could not be taken nearer than to about 1 per cent. at the end of the range, and 3 per cent. towards the centre. Of course if currents of only three or four milli-amperes are to be measured, these can be got with at least this accuracy by using the coil at some convenient point near O, A and B being rather far away. As an accuracy to about 5 per cent. is all that is requisite in measuring currents for hospital work, there can be no doubt that this instrument fulfils the purpose for which it was made, having an accuracy in use of at least five times this amount. I have to thank the assistants in my laboratory for the excellency of their workmanship.

XXXV. *The Measurement of High Specific Resistances. By* R. THRELFALL, *M.A., Professor of Physics, University of Sydney, N.S.W.*[*]

[Plate VIII. fig. 3.]

THE experiments which form the subject of this paper were begun almost immediately after my arrival in New South Wales, in June 1886, and have been continued at intervals ever since. The original object was to measure accurately the resistance of certain gums produced by trees growing in the Colony. The only gum thoroughly examined however, up to now, is that produced by the "grass tree" (*Xanthorrhœa hastilis*). This gum, in spite of many attempts to improve it by various methods of purification and by mixing with other substances, turns out to be useless as an insulator, having in fact no higher resistance than, say, ordinary samples of resin, that is about $4{\cdot}1 \times 10^3$ megohms per cubic centimetre. Besides this, the gum in question is faulty in other ways. It is of the nature of shellac, but cannot compete with the shellac as ordinarily supplied either in price or purity. In addition it has the two fatal defects of being partly soluble in water and of decomposing before it melts. Long-continued gentle heating does not seem to improve it in this latter respect; while the texture of the material becomes looser, it grows friable and very dark in colour. Benzoic acid appears to be given off in large quantities during the process. In spite of many attempts I have hitherto failed to obtain any considerable quantity of the fig-tree gums which are said to be produced in large quantity in the northern parts of the Colony. This paper therefore will be devoted to a description of the method adopted in measuring these resistances, a method which ultimately reached a considerable degree of perfection; partly on account of the modifications introduced in the construction of high-resistance galvanometers, and partly on account of the peculiar property of Clark cells. The method has since been employed for other measurements, as will be shown later on.

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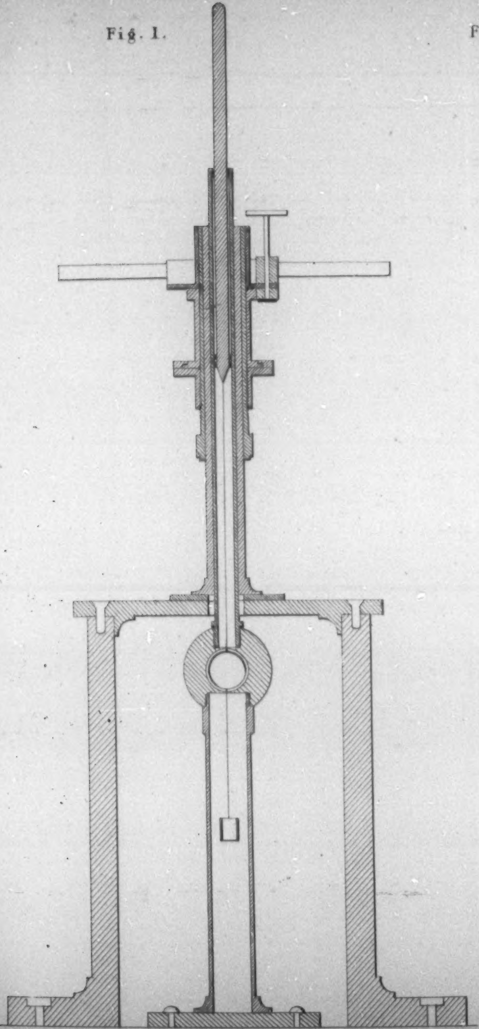
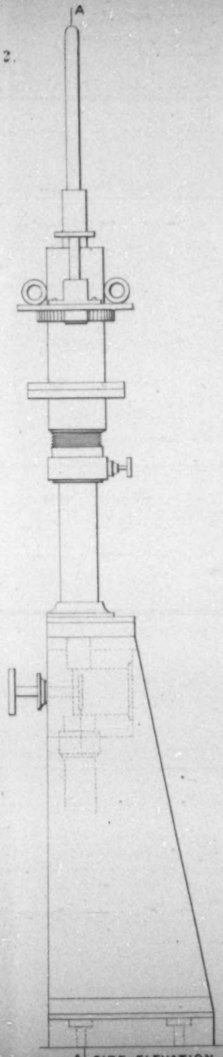
[*] Read March 23, 1889.

Fig. 1.



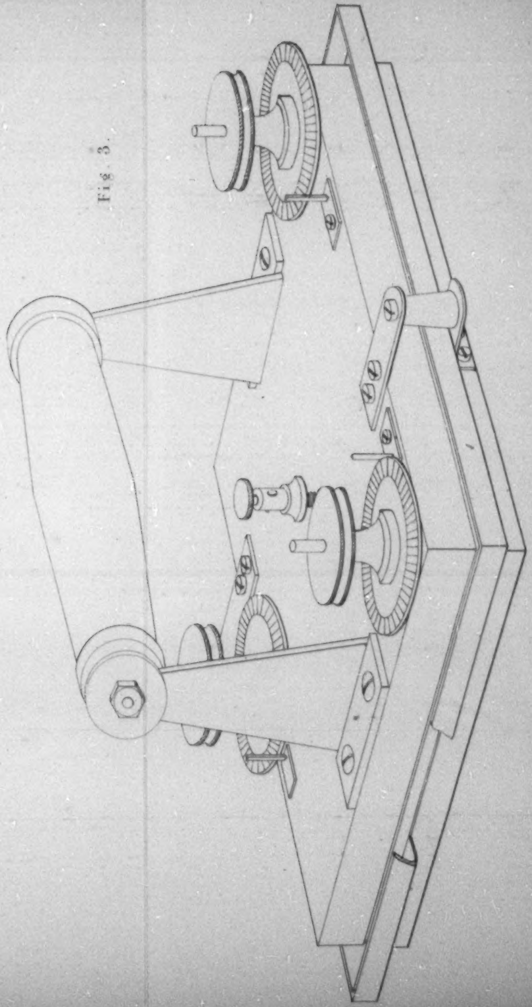
SECTION ON A. A.

Fig. 2.



SIDE ELEVATION.

Fig. 3.



Mintern Bros. lith.

I do not wish to claim any superiority for the galvanometer over the electrometer methods, except that, given the galvanometer and cells, it is certainly more easily applied, especially when the determinations are numerous. I was forced to adopt the galvanometer method in this instance through not having an electrometer ; but I was by no means unwilling to do so, because I have long been of the opinion that the galvanometer as usually constructed is susceptible of considerable improvement for delicate work by simply pushing the ordinary conditions of sensitiveness nearer to their limit.

GENERAL DESCRIPTION OF METHOD.

Measurement of Resistance of Gums.

The substance to be tested forms a layer of very exactly estimable dimensions between two plates of conducting material. The experiment consists in obtaining equal deflexions of a sensitive galvanometer—(1) when a known fraction of the E.M.F. of a Clark cell is allowed to act through a megohm in the galvanometer circuit ; and (2) when the E.M.F. of a known number of compared Clark cells is allowed to act through the resistance to be measured.

The apparatus, therefore, consists of the plates with the substance to be investigated, the galvanometer, the standard cells, and a megohm.

The Resistance-plate Arrangement.

This consists of two brass plates accurately rectangular and scraped flat on one surface. The dimensions of the plates I used were—length 15·2 centimetres, breadth 12·7 centim., thickness 1·6 centim. These rather exact numbers were obtained by filing. The measurements were made (α) by beam compasses, (β) by the dividing-engine. Neither the corrections which had to be introduced for temperature nor the comparison of the dividing-engine scale and the beam compasses with the standard metre are given, as no absolute measurements of pure substances have been made. Several measurements of each plate were made by both methods. The surfaces were made flat by scraping, and this process was continued till the contact was sufficiently perfect for one plate to lift the other when laid on it, both surfaces being clean in the ordinary sense. The upper plate is furnished with a solid handle something like the handle of a flat-iron, and is pierced by three holes, through which pass the micrometer-distance screws (see Plate VIII.). The screws are 4 centim. long, and the threaded portion is ·55 centim. in diameter (they would have been better if twice the diameter). The micrometer-heads are divided into a hundred parts each, and the mean pitch of the screw, as determined by a comparison with the millimetres of a standard scale by means of a measuring microscope, is 39·5 divisions to a millimetre; that is, one turn is equal to ·5063 millimetre at 20° C. The points of the screws are conical, and the distance from the end of the thread to the point of the screw is 5 centim. The screws are of steel tempered to the blue, the heads of brass; and the tap used to produce a thread in the holes through the brass plate was identical with one of the screws; the lathe being set to use the same part of its screw and of its change wheels during the making of each. The measurement showed that the screws were very good especially in the middle portion; they were also practically exactly alike. The goodness of the fit in the brass plate was shown by the fact that an increase of temperature of 20° C. was sufficient to "bind" the screws very perceptibly. Indices similar to the indices of spherometers were erected in the upper plate of the apparatus—one index for each screw. All the workmanship being accomplished, the plates were next platinated by a process given in Gore's 'Electro-metallurgy' under the name of "Roseleur's Process;" a previous experiment showed that when the directions are faithfully carried out, this process will yield a hard bright deposit of platinum. The bath, however, is very troublesome to keep in order since no solution of platinum takes place to supply the place of that deposited. To prepare the plates for platinating, they were first heated to the temperature of boiling water and rubbed on the scraped surfaces with a solution of caustic potash. Finally, they were rubbed with a bit of fine pumice dipped in dilute caustic potash. This is by far the best laboratory method known to me for preparing surfaces of brass for electro-plating; a clean surface is obtained with comparatively little abrasion. Before I found this out, I was much troubled to secure a good deposit.

After platinating, care being of course taken to prevent the deposit being unequal, the plates were carefully washed and dried. It was noticed that the metallic surfaces were covered with a faint bloom of black platinum. On placing the plates together and moving the top one slightly, the lower plate at once adhered, and though weighing several pounds could easily be lifted by the upper one. On pulling the plates apart the "bloom" was found to be burnished practically all over both surfaces, showing of course that the platinating had not sensibly altered the planeness of the surfaces. For this accuracy I am much indebted to the university assistant, Mr. James Cook, who, being accustomed to prepare optically flat surfaces, was led by the application of experience gained in that way to the happy result above mentioned.

The exact position of the two plates with respect to one another was secured by cementing the plated surfaces together with hard paraffin. Two lugs of thick brass were made fast by screws to each plate, and brought as near to one another as consistent with leaving an air insulating space between them. These were then bored to fit slightly conical steady pins. A rim was also screwed round the lower plate, so that when the top plate was removed the bottom plate resembled a tray. The rim projected about ·8 of a centimetre on each side of the surface, and extended to a height of about half a centimetre above it.

The gum was introduced between the plates in the following way. The micrometer-screws were carefully cleaned and screwed through their holes till they made contact with the lower plate; the point of contact was almost as easy to estimate as in the ordinary use of the spherometer. At all events three or four consecutive attempts to fix the point of contact did not differ from each other by more than about one half of one of the micrometer-divisions. The accuracy with which the contact-point can be fixed depends mainly on the workmanship of the screw, which must fit perfectly "tight and free," to use the mechanic's very expressive term. The contact-points having been found, they were permanently scratched on the micrometer-heads and called zero points. The next operation consisted in screwing each of the screws through a known number of turns. In one experiment this amounted to making the distance of the plates apart = ·02 centim.; and in another the distance was reduced to ·01 centim. with equally good results. It is not advisable in any absolute measure to reduce the distance to much less than this, because the error of the micrometer, depending as it does (as in this case) chiefly on the small irregularities of the screw, must not be allowed to become sensible. There is no doubt, however, that with first-rate appliances the micrometers might be easily made a hundredfold as accurate as mine, and their travel actually measured *in situ* by a suitable reading-microscope. In this case it would be important to make the screw portion much thicker to avoid any risk of permanent distortion (twisting) when the screws are finally screwed back while partly held by the gum*.

The screws being adjusted, the plates are slowly heated in a gas-oven till some gum laid on the surface of the lower one is in a state of quiet fusion. The great object is to avoid any distortion of the plates. With this aim in view the plates described were cast about twice as thick as they finally required to be in order that the "shell," supposed to be in a different state of strain to the interior, might be approximately removed. The plates were planed first on one side and then on the other till the right quantity of metal was removed; the last cuts being taken very fine. The handle affixed to the upper plate was of course arranged so as to fit loosely, and not in any way constrain the free expansion and contraction of the plates; the temperature having been often violently changed (by heating for cement &c.), it is hoped that the plates may be considered fairly well annealed. There is no doubt, however, that for complete satisfaction in an absolute measurement the plates should be capable of being optically examined during the process of heating. This would require to be done in the gas-oven or other uniform field of temperature and at the time the experiments were made. I had not the requisite appliances.

It was found by several trials that the best way of obtaining a layer of gum free from bubbles between the plates, was to float the lower plate or tray pretty full of gum, and also to obtain a layer of gum free from bubbles and in quiet fusion on the upper plate. In the case of the grass-tree gum this could only be obtained by heating the gum for some time at a temperature higher than the one at which it was when the plates were brought together. This was accomplished much in the same way as is sometimes done in microscopy—when the cover-slip is placed on the slide with one edge down and the other end gradually lowered. The freedom from bubbles of the layer of gum obtained in this way was tested by allowing the plates to cool, and then heating the lower one till its surface reached the temperature of the melting-point of the gum, the upper one being kept cool. This being done the upper plate could finally be lifted, leaving only a small portion of the gum on the lower plate—owing to the small heat-conductivity of the gum. The layer of gum was left thick on one occasion for the purpose of this test, and when the manipulation described above was properly carried out there were no bubbles; the layer in fact was very homogeneous indeed. The only danger left so far as the insulating material is concerned is that it may tend to crack away from the plates during cooling. In the cases examined this did not seem to be the case, because in the first place a thick layer of the same substance rapidly cooled on a thin plate showed no tendency to crack. Again, a great many insulating substances are more or less plastic, or rather viscous, down to temperatures very nearly approaching those at which experiments are usually made (in this country 23° C. is a not unusual temperature). Again, the massiveness of the plates being considerable no distortion of them ought to be caused by the gum, if the ordinary precaution of allowing them to cool slowly be observed.

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* Since this was written an improvement of the above micrometer-screw has been devised, entirely getting rid of the difficulty here referred to.– Oct. 3, 1889.

In the experiments hitherto made the cooling took place during the night in the gas-oven, which being coated with non-conducting material took a long time to cool. On the other hand, in all experiments of the sort one is in a dilemma. If the substance is placed between conducting-plates there are dangers of the kind mentioned; if, on the other hand, the material itself be worked with a view to making it take a prescribed form, the difficulties, especially in the measurement of its thickness, become great. I began by making some attempts of this kind, using blacklead to make the gum-surface conducting, and plating this electrolytically. The difficulties arising in the shaping of the plates are, however, practically prohibitive with friable material. Besides this I have often noticed that in electrotyping it is difficult to prevent the deposit being "spotty" at first, and this has shaken my faith in the perfect continuity of ordinary blacklead surfaces. Possibly platinating with an induction-coil may be really the best way.

However, to finish the description of the case in point:—The gum was carefully scraped away from the edges of the plates as soon as they were cool, and the screws were screwed back. This could not be done with any ease at first because of the cementing action of the gum. This was got over by heating the head of the screw with a Bunsen flame: finally, the screws were retracted far enough to be quite out of the way*. A correction to the area of surface has of course to be made for the three screw-holes. If the thickness of the gum be considerable compared with the diameter of the hole, this may be very complicated. In the present case it was negligible. Thus the whole area of the gum-plate was 193·04 centim. less the area of the three screw-holes —·7128 centim., *i. e.* 192·3272.

Now it is clear that, owing to the curvature of the lines of flow round the edges of the holes, the real correction will be less than the one made. The deposit of gum, however, is pierced by a hole corresponding to the conical end of the screw and, consequently, only very small.

Finally, the steady pins are taken out of the plates and the gum is ready to be measured.

In order to measure the resistance of good insulation by means of this arrangement, it is clear that it will be advantageous to have a galvanometer of the highest degree of sensitiveness. This is desirable both because the thicker the insulating layer the less will be the experimental error in the determination of its thickness; and the smaller the electromotive force required the less will be the difficulty of estimating it exactly, as will be shown in the proper place. I therefore attempted to obtain sensitiveness by pushing the ordinary conditions further than is usually done. My first experiments were on a galvanometer of about 9000 ohms' resistance, made by the Cambridge Scientific Instrument Company. It was soon very evident that when the current reached the value of about 10^{-7} ampere, the torsion of the suspension became important. My first modification was to increase the length of the fibre to about 12 inches; this led to considerable difficulty of adjustment, but increased the sensitiveness about fiftyfold. It then became clear that the next step must be to get the magnets more perfectly astatic and to reduce the weight of the mirror. The reducing of the weight of the mirror turned out to be more difficult than I anticipated; however, it was finally arrived at, and at the same time the astaticism was made more perfect. Some experiments showed that it was very difficult to get two sets of steel bars of the kind ordinarily employed even reasonably astatic. The difficulty lies partly in the magnetizing and partly in obtaining exactly equal quantities of steel in the two systems of magnets. In fact it is necessary that the steel bars be magnetized *in situ*, otherwise they can hardly be perfectly arranged and are sure to demagnetize each other more or less. Now when the two magnet systems are only separated by a bit of aluminium, say three inches long, it is impossible to thoroughly magnetize one system without demagnetizing the other more or less. Consequently it is necessary to set up an arrangement so that both systems can be magnetized at once. The following is a description of the arrangement adopted:—It consists of two small electromagnets with extremely soft cores, and movable pole-pieces most carefully worked so as to fit the ends of the cores. Every precaution was taken to make the electromagnets as much alike as possible; the iron was cut off the same rod, it was bent to the same templet, the annealing of both cores was done in a box of asbestos at the same time. The four brass bobbins carrying the wire were also made as much alike as possible, and the same number of turns of wire were put on each bobbin by means of a revolution-counter. The winding was quite uniform, No. 18 B.W.G. wire being used. As a check the resistances of the bobbins were measured, when it fortunately happened that two were about one half per cent. higher than the other two, and so they were paired. The wire, it need hardly be added, was wound on to the four coils under a constant strain. The two electromagnets were then mounted on a permanent stand—one being kept steadily in a fixed position, and the other being capable of sliding parallel to a line drawn perpendicular to the lines joining the centres of the poles of each magnet. The pole-pieces were bevelled off from the top side; but the area of the ends remained large compared with the size of the magnets to be magnetized. The condition as to equality of quantity and quality of steel in the galvanometer magnets was next considered. After some reflection I decided that the most probable way of securing equality would be to discard bar-magnets entirely and use disk-magnets. I therefore procured a small piece of sheet steel about as thick as ordinary thin writing-paper, and had a die constructed so as to stamp small disks from this sheet. The sheet was fairly hard, and it was found that the disks "stamped" better when the sheet was taken in its natural state than when it was softened. A considerable number of disks were stamped out of the sheet, and these were then laid on a bit of flat iron and raised together to a bright red heat; they were then plunged together into a jar of cold water. On examination they all seemed to be glass-hard, and some of them remained flat. The four flattest ones were chosen and prepared for mounting. A bit of aluminium wire was cut to the right length and beaten out flat at each end. The disks were then cemented with shellac varnish, one on each side of each flattened end of the aluminium wire. The wire was thus much more accurately the centre of rotation of the magnetic system than is generally the case. Attempts were then made to get a good light mirror. About three ounces of small microscope cover-slips were examined by aid of the reflected image of the bars of a window, and from these about twenty were selected and silvered by the Rochelle salt process. They looked very good, but on mounting for trial without strain they all turned out disappointing. I finally made use of a small portion of a larger mirror that had got broken. This was cemented on to the flat surface of one of the steel disks and was found not to be sensibly distorted. After trying very many cements, I incline to think that nothing is better than a trace of slow-drying white paint. Amongst other experiments I tried cementing two very thin glass disks together, selected so as to mutually correct each other by the drying of the cement. I also tried a method of using plaster of Paris. As plaster of Paris expands on setting I covered the back of a thin mirror with a layer of it about $\frac{1}{12}$ in. in thickness on drying, this of course forced the mirror into a concave form. The back of the plaster was rubbed away on a fine file till it was only about $\frac{1}{20}$ in. thick, and the mirror still remained very concave. Since cementing magnets on to disks with shellac varnish invariably forces the mirror to become convex, I hoped that I should obtain a correction of the concavity produced by the plaster by the convexity which the shellac tends to provoke. My anticipation was completely realized ; the mirror on examination turned out all that could be desired, but, alas, was too heavy for the purpose for which I required it. I can, however, most strongly recommend the process to anybody who desires a mirror to be flat and does not mind it being heavy. The best way is to use very little plaster and then to leave the mirror concave. This concavity can be removed by painting on small successive films of shellac ; it must be remembered that shellac films go on contracting for several days after they cease to be sticky. Equally good results can of course be obtained by cutting out (with a rotating tube and emery) disks of the size required from previously examined thin sheet glass. The surfaces generally require regrinding. The advantage of the process described is that it enables thin cover-slip glass, which is generally to hand, to be kept flat.

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* NOTE, Dec. 1888.—The difficulty is, however, serious, and has led to new hollow screws being made. Through the holes bored down the centres of the screws gold-plated copper rods pass; these are pinned to the screws till it is required to retract them; the ends of the screws themselves are flush with the lower surface of the upper plate, or very near so.

The mirror having been mounted on the steel disks, these last were magnetized by the apparatus mentioned above. In carrying out this operation the following precautions have to be observed:—

1. The distance between the pole-pieces requires to be the

same for each magnet. This was attained by setting them to touch a carefully prepared brass rectangular bar.

2. To annul the effect of any small outstanding differences between the cores of the magnets the current was supplied to them in multiple arc, and was strong enough to magnetize the cores beyond the saturation point.

The approximate moment of inertia of the magnet system was easily calculated, and it was found that the astaticism was at least ten times as good as the best I had been able to obtain with small bar-magnets mounted on mirrors or mica, and magnetized with a small horseshoe magnet. Of course, as has been pointed out by several observers, there is danger of rapid variation of the magnetization; but it was thought better to risk this than to force it by artificial "ageing" by heating or otherwise. As will be seen hereafter, the magnets were never exposed even during the experiments to anything but the smallest electromagnetic forces, and the controlling magnet was weakened and introduced from high above the galvanometer, and only lowered sufficiently just to make its influence on the combination really felt. This is a delicate operation with ordinary arrangements, but becomes simple when the construction of the galvanometer is modified in a way to be explained directly. If one desires to keep the astaticism perfect, it is necessary to be mindful not to use the controlling magnet so as to produce demagnetization; nor must the currents through the galvanometer ever rise to much greater values than those corresponding to the effects to be observed. From an examination of the investigation in Maxwell, vol. ii. articles 437 and 438, both Mr. Adair and I came to the conclusion that the disk form of magnet would retain its magnetization pretty well. This has turned out to be the case, for after more than a year's hard use, for all sorts of purposes, the galvanometer has still a sensitiveness of about one division for 10^{-9} ampere. The galvanometer is in daily use for testing cells with a view to their application to the resistance measurements at present under discussion.

This galvanometer, however, never came to be relied on to measure currents of less than 10^{-8} to 10^{-9} ampere. In the course of reading on the subject I consulted the paper by Messrs. T. and A. Gray in the Proc. Roy. Soc. 1884, vol. xxxvi.

p. 287. These gentlemen made use of a new arrangement of magnets and coils, which, however, can hardly be understood without referring to the picture, *loc. cit.* The coils and magnets were so arranged that the poles of the magnets were normally situated in conical holes containing the axes of the coils. The two horseshoe magnets were suspended from a frame of aluminium wire by one or two silk fibres of considerable length. The coils themselves were composed of very fine wire and had a high resistance. I lost three months' hard work in making and testing this arrangement, which certainly has the advantage of being practically perfectly astatic. The suspended arrangement being rather large was most troublesome to mount and balance, and had the additional disadvantage of having so great a moment of inertia that its period of vibration often amounted to 70 or 80 seconds. This sluggishness had the property of making it most difficult to use, for it was hardly possible to distinguish the motion due to the electromagnetic forces from the never-ceasing motion due to air-currents. Though the instrument was well-protected by a glass case, and this generally supplemented by a wooden box, I never succeeded in eliminating the effect of air-currents, though it must be added I never succeeded well enough with it in other respects to make it worth while to apply the "subjective" method of mirror observation. In any case it seems to be essential to have a means of adjusting the coils to the magnets as well as the magnets to the coils; but the adjustments are very tiresome even with the facilities which in the later forms of this instrument I had for making them. My coils had not quite so much wire as those of the Messrs. Gray because I used all I had and could get no more in Australia. However, the aggregate number of turns amounted to 59,900, and the resistance at 20° C. was 15,852 B.A. units, as against 62,939 turns and a resistance of 30,220 ohms attained by the authors quoted. The authors also state that the wire was approximately uniformly distributed throughout their coils, though in my case this was found impossible, keeping the external dimensions quite constant, and therefore there was a slight difference between the coils, which, however, was compensated for by their arrangement in the instrument. I do not think that the diminution of the number of turns had much effect, because the diameter of the coils had reached 5·8 centim. Feeling that my non-success was probably to be traced to my inferior skill as an experimentalist, I undertook a long series of trials with a view to discovering the best way of hardening the magnets and their best position in the coils when at rest; amongst other experiments the following will do for description. Three bits of Stubb's steel wire were carefully cut and filed to a uniform length of $4\frac{1}{16}$ in., their diameters being $\frac{3}{64}$ in. These will be called A, B, and C. A was made glass-hard throughout; B was hardened through a distance of from $\frac{3}{4}$ to 1 inch at each end; C was hardened from a distance of from $\frac{1}{8}$ to $\frac{1}{4}$ inch at each end. These bits of wire were then magnetized by being placed between the massive poles of a very large electromagnet. The cores of this magnet were 3 inches in diameter and about 25 inches long. The pole-pieces were very broad and thick. The magnetization of the steel was carried nearly to saturation, and the magnets were found by filings to be free from consequent poles. On taking the times of vibration the following numbers were obtained:—

A made 25 vibrations in 125 seconds.
B " " 124 "
C " " 105 "

After remagnetizing the magnets with about half as much current again round the electromagnet, it was found that

A made 25 vibrations in 123 seconds.
B " " 107 "
C " " 107 "

B therefore was improved; A and C remaining about the same. The deviations were probably produced by the unavoidable shaking and jarring in mounting the magnets, though this was done with considerable care.

These magnets were next observed with respect to their behaviour with one of the coils. Coil "No. 3" was selected for this purpose. It was placed on the table, and above it hung a specially fine spring-coil of wire forming part of a Jolly's balance. The magnets were hung from the end of this spring by a loop of silk, and could be adjusted to penetrate the coil to a greater or less extent. In general five positions were taken:—

Position 1. Magnet-end flush with the upper windings of the coil.
„ 2. Magnet-end at ¼ of the length of the hole through the coil.
„ 3. Magnet-end at the centre of the coil.
„ 4. Magnet-end at ¾ of the length of the hole through the coil.
„ 5. Magnet-end flush with the bottom of the coil.

Three Leclanché cells were allowed to run for four hours through the coil before the experiment began. The coil had a resistance of 3130 ohms.

First, a series of observations was taken by observing on the glass scale the equilibrium position of the magnet with the current direct and reversed. The magnet was then lowered to position 2, and so on; then it was reversed and the experiments repeated. Two complete sets each way were made for each magnet, *i. e.* forty observations of distance and forty reversals of current. The experiment was a very pretty one, and I never remember to have seen any apparatus work better. The following set is given as a sample from the notebook. The numbers, of course, have no significance except with respect to the actual coil and magnet taken.

Magnet B.

Position in Coil.	Double displacement with current + and −.	Number of experiments.
At edge of coil . . .	·43 centim.	3
¼ way in	·77 „	2
½ „	1·03 „	2
¾ „	1·159 „	2
Near bottom of coil . . (⅛ inch off)	1·04 „	2

Magnet B reversed, otherwise everything the same.

Position.	Displacement current + and −.	Number of experiments.
At edge	·415 centim.	2
¼ way in	·78 „	2
½ „	1·085 „	2
¾ „	1·27 „	2
⅛ off bottom	1·11 „	2

The net result was that all the magnets behaved best when they started from three quarters to the whole way in, and that B was best, C almost as good as B, and A distinctly the worst. On reducing the observations it turned out that the ratio of the mean displacements of A and B was about ·782, while the ratio of the magnetic moments was ·755. This relation is of about the order one would expect, seeing that the length of the magnets was about 9·6 centim. and the dimensions of the coil were :—axial 3·8 centim., radial at one end 2·4, radial at other end 2·5. The external surfaces of the coils were cylindrical.

The ratio of the greatest displacements was ·643. It may be conceded, therefore, that the questions of magnetization and placing received a fairly complete answer. In the final arrangement of the galvanometer with horseshoe magnets, like those described by the Messrs. Gray, the length of the yokes of the magnets was 8·5 centim., and thus this was the distance between the centres of the coils. The legs of the magnets were 3·8 centim. long, and they were magnetized like the trial magnet B. When in position, the magnet-legs projected into the coils to an extent of about $\frac{7}{8}$ the axial dimension of the latter. The suspension was two washed silk fibres (one would not carry the load) 16 centim. long.

The test for sensitiveness was made by running a large Clark cell (already described) through 10,000 legal ohms, and a certain small resistance taken out of an ordinary Bridge-box. The terminals of the Bridge-box were coupled up through the galvanometer to a megohm. The accuracy of this method of testing has been already established.

The distance from the galvanometer-mirror to the scale was 155 centimetres; and the light-spot was very good, showing the wire image as sharply as the lines on the scale. The scale itself was divided into millimetres. In the final test the period of vibration of the magnet system was 80 seconds, and the resistance out of the Bridge-box was 100 ohms. The E.M.F. acting through the megohm and galvanometer and 100 ohms was therefore $\frac{100}{10100}$ Clark cells, say ·0145 volt. The current was therefore $\frac{\cdot 0145}{116000} = 1\cdot 26 \times 10^{-7}$ amperes.

Employing the method of vibrations and neglecting the correction for the extremely small log. decrement, the battery being of course reversed and several experiments made, it was found that the double deflexion amounted to 5 scale-divisions (millims.). Hence the deflexion corresponding to $1{\cdot}26 \times 10^{-7}$ amperes is 2·5 divisions. Now I do not think that, bearing in mind the lengthy period of the system, it would be possible to read to more than ·5 division. The difficulty comes in in eliminating air-currents; as far as the scale went I could read certainly ·2 division, so there is no advantage to be gained in having the scale further, or even so far away. We may therefore say that five times the least measurable deflexion is given by $1{\cdot}26 \times 10^{-7}$ amperes, or that the least measurable deflexion itself is given by $2{\cdot}5 \times 10^{-8}$ amperes. This, I may mention, is only to be obtained when the magnets are judiciously kept from swinging by an extra controlling magnet, worked carefully from a distance till the vibrations get small, so as to jam against the side of the coil. Some fine quartz threads were also prepared by Boys' method, but no appreciable improvement introduced by their use could be detected. This may, however, have been on account of their thickness; for they could be seen, with a little practice, in a good light, and when laid on a bit of black silk. I therefore came to the conclusion that neither I nor my instrument-maker (who is fairly good) could hope to compete in such delicate work with the Messrs. Gray, who, using this type of galvanometer, attained a sensitiveness of one half millimetre-division with a current of about 10^{-11} amperes, and that with a scale fairly close to the instrument, and with a manageable period of vibration.

In consequence of this failure I determined to use the four coils constructed for the Gray galvanometer, for an experiment in which they were employed in the usual manner. They were therefore roughly mounted, and provided with an astatic combination of the kind previously described. The results were so encouraging that the same methods were pursued further. In the final form the suspension was a quartz fibre, 85 centim. long, suspended in a carefully chosen glass tube. With such long suspensions the tube must be very straight, and the arrangement for raising and lowering the suspended parts must be very good. After trying several arrangements for this, I adopted a pointed piston working into the tube and passing through a stuffing-box. The coils were of course anything but suited, as far as shape goes, for their present arrangement; however, they did what was requisite, though I have no doubt that coils might be made to increase the sensitiveness tenfold.

The mirror was in this case suspended midway between the magnets, and, for want of a better, was so thin that it got a little pulled out of shape by the paint which was used to fasten it to the aluminium wire. This fortunately turned out to be an advantage in some respects, for the vertical wire and the paint on the back of the mirror, by a happy accident, made the figure of the mirror practically that of a portion of a cylinder with a vertical axis; consequently, using a very good lens of 40 inches' focus, a good image of the light-spot was obtained at a distance of three metres. It became evident at once that the two real difficulties in securing sensitiveness lay in preventing air-currents and in adjusting the controlling magnet.

The first was finally attained by making the instrument practically air-tight; and, by means of a diaphragm, stopping down the beam of light to very nearly the size of the mirror —in this case of about 1 centimetre diameter. During the measurement of the resistance of an impure sample of sulphur this protection against air-currents was found to be insufficient, and the galvanometer was further protected by enclosing it in a cardboard box. If it ever becomes necessary to make an instrument to be sensitive to, say, 10^{-13} amperes, I shall have the support for the controlling magnet absolutely independent of the galvanometer-case, so that any vibrations set up in adjusting the magnet shall be transmitted only through heavy masonry. In the galvanometer now being described the controlling magnet could be raised by a nut and screw combination, itself sliding on a brass tube attached to the case of the instrument. The glass tube containing the fibre was clamped at its upper end to a very heavy stand of brass and lead, and this practically sufficed when the sensitiveness got to be of the order of one division to 10^{-11} amperes.

The arrangement for adjusting the distance of the controlling magnet was arrived at after several trials It consisted of an apparatus sliding and clamping on the brass tube, with a nut and screw for fine adjustment. It was intended to use a worm-wheel and screw for the adjustment in a horizontal plane; but this was found after a little practice to be unnecessary, although it would be convenient.

The details of this arrangement for raising and lowering the magnets will be understood from the drawing of the galvanometer for medical purposes on Pl. VIII. figs. 1 and 2, see p. 247, *suprà*. Many experiments were made in order to decide the relative merits of quartz and silk fibres. At first it was thought that silk did as well; but after a time a great deal of trouble with the zero was traced to the silk, and attempts were made to use finer quartz threads. In this, owing to the skill acquired by Mr. Pollock, I was finally successful. As I have had about a year's experience in drawing quartz threads, I add the following notes on the process in hope that they may prove of use to others. The difficulty is to get a large enough bit of quartz fused onto a suitable handle. The best way of managing this is first to heat a bit of rock crystal red-hot in an ordinary crucible and keep it heated for about an hour. On cooling, it will be found to have split into fragments of all sizes; one of these is chosen, and supported on a bit of lime or on a massive bit of iron, and is then fused under the oxy-hydrogen (not oxy-coal gas) jet. When it has once been got glass-like it never cracks again, no matter how suddenly it may be heated. Porcellanous quartz draws into rotten threads, as might be expected. Two bits of fused quartz having been prepared they may be fused to the ends of two bits of clay tobacco-pipe, and can then be manipulated in the oxyhydrogen flame without trouble. There is no difficulty (when once the short thick threads have been drawn by hand) in the subsequent shooting. I most cordially indorse all that Mr. Boys says in favour of this admirable invention.

Another difficulty lies in the obtaining of a reasonable degree of astaticism. It has already been shown that it is practically possible to increase the astaticism of a magnetic combination by careful methods of magnetization and manufacture of the magnets: but the astaticism thus in general obtained is by no means perfect. The investigation of this matter was undertaken by Mr. Adair, and proved to be difficult and unsatisfactory. In the first place it was necessary to determine the coefficient of torsion of the silk fibre to be used in the experiments. This fibre was about thirty inches long, and before mounting had been boiled in a tube of water. A copper disk, made up with a mirror so as to have about the same weight as nearly as possible as the astatic combination to be examined, was suspended from the fibre in a vibration-box furnished with a long glass tube. The copper ultimately employed was supposed to be electrolytically pure. The combination at first was slightly diamagnetic, but became much less so as the paint-cement dried, and was finally almost indifferent to any means we could find for testing it. From experiments with this disk, whose weight and moment of inertia were known, the coefficient of torsion of the fibre was found to be T=·000115 C.G.S., with a load of ·287 gram.

Two astatic combinations were next mounted and tested. The first was the one that had already done some work in the galvanometer, the second was carefully made for the purpose. The moment of inertia of the first was found to be ·02126, and of the second ·03274.

With both combinations two sorts of experiments were made. The time of vibration of each was determined, and the change of zero produced by twisting the upper end of the fibre through a known angle, generally 2π. From these well-known methods it was found that the systems had a period of vibration of about 2·51 seconds only. This corresponded to a value for the moments of the forces of about ·1270. The magnitude of this number, as well as the positions taken up by the combinations, showed not only that the astaticism was far from perfect, but also indicated the cause of this: the magnets were not really in one plane. Tentative twisting of the aluminium wires was then resorted to, with the result of bringing up the periods of vibration to 11 and 12·8 seconds respectively. In this latter case the moment was reduced to ·0064. The needle that had been brought to a free period of 11 seconds was mounted in the galvanometer, and by means of the controlling magnet was brought to have a period of 36 seconds, corresponding to a magnetic moment of about ·0007 C.G.S. During the experiments on resistance the period was got considerably longer than 35 seconds.

The next paper, on the Resistance of Impure Sulphur, contains the details of the method employed to find both the specific resistance of the gums mentioned and of sulphur. As no useful result is to be expected from a publication of the long series of experimental numbers obtained in the work on Gums, I refer to the following paper for the description of the *method* employed, since it remained constant throughout.

XXXVI. *On Measurements of the Resistance of Imperfectly Purified Sulphur.* *By* Prof. RICHARD THRELFALL, *and* ARTHUR POLLOCK, *Esq.**

THE galvanometer having been brought to a state of sensitiveness of 5 scale-divisions for 10^{-11} amperes, the measurement of the resistance of the sample of sulphur in question became a tolerably easy matter. The sulphur had been supplied by Messrs. Hopkin and Williams as "precipitated, washed." It looked clean when melted; but on examination turned out to have the following substances existing as impurities:—

Calcium sulphate.
Ferric oxide.
Organic matter.

Dr. Helms, Demonstrator of Chemistry in the University of Sydney, was kind enough to investigate a sample of this sulphur with a view to discovering whether it contained selenium or tellurium. The result of his examination of about two hundred grammes of the substance was, that neither of these substances was present in quantity large enough to be detected. The examination was carried out by means of the oxidation and sulphurous acid method; and also by the cyanide method. We desire to express our thanks to Dr. Helms. The importance of this result, so far as our work is concerned, lies in the fact that it shows that pure sulphur can probably be obtained from the sample at hand by means of distillation. The following measurements refer to the unpurified sample; the only substance existing in any considerable quantity was calcium sulphate. The sample was probably rather more pure than ordinary "roll" or flour sulphur. The general arrangement of the apparatus will be clear from the following diagram.

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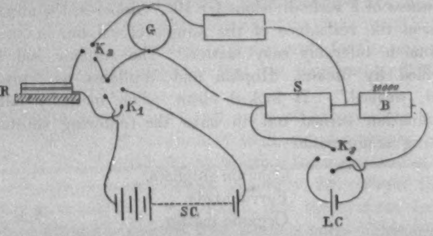
* Read March 23, 1889.

By means of the key K_1 a current can be sent in either direction through the resistance R and the galvanometer G. The source of this current is a suitable number of small Clark cells SC. The E.M.F.'s of these cells and of the large one LC were watched during the experiments with the aid of an auxiliary galvanometer and balance arrangement not shown in the diagram. The key K_2 allowed the upper metal plate of the resistance arrangement to be put in communication with the last cell so as to charge up without allowing the current to rush through the galvanometer. After charging, the connexions were altered so as to allow the current due to the leak through the sulphur to pass through the galvanometer. The E.M.F. of the small cells was not changed by this amount of leaking, and the fall of the large cell was measured at the time in the usual manner. The experiment consisted in taking plugs out of the box S till the galvanometer gave the same deflexion whether the current was sent through it by the number of cells through the sulphur, or by the adjustable fraction of the E.M.F. of the large cell through a megohm. The sum of the resistances of S and R was always 10,000 legal ohms. The arrangement of the observations was such that they interlocked in time.



The absolute value of the result, therefore, depends chiefly on the value of the megohm. Respecting this standard I wrote to Messrs. Elliott to inquire what degree of reliance could be placed on it, and was informed in reply that it had been tested against a standard in blocks of 100,000 ohms each, and was right within the variation produced by one degree of temperature. The wire was of German-silver. As the present results are interesting only with respect to the method and the resistances of the sample under varying E.M.F.'s, the absolute value of the megohm is of comparatively small importance.

The following table gives the data of the experiments on the resistance of the sulphur:—

Date. 1888.	Temperature of room.	No. of small cells used.	E.M.F. of set of small cells.	E.M.F. of large cell short-circuited through 10,000 L. ohms.*
	°		volts.	volts.
Oct. 20 ...	17·0 C.	20	28·661	1·432
20 ...	17·0 C.	40	57·312	1·432
21 ...	16·2 C.	40	57·312	1·432
21 ...	16·2 C.	20	28·661	1·432

Date. 1888.	Resistance taken out of S.	Thickness of layer of sulphur.	Area of layer of sulphur.	Resulting specific resistance of the sulphur.
	L. ohms.	centim.	sq. centim.	
Oct. 20 ...	9·0	0·05	189·8	$8·575 \times 10^{13}$ l. ohms.
20 ...	15·0	0·05	189·8	$10·286 \times 10^{13}$,,
21 ...	10·7	0·05	189·8	$13·307 \times 10^{13}$,,
21 ...	5·8	0·05	189·8	$14·425 \times 10^{13}$,,

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* Lord Rayleigh has kindly pointed out to us that by a mistake in our method of reduction we have slightly underestimated this value. The result will be that the absolute value of the resistances as given are very slightly too large.

The following is a sample of the readings taken:—

Deflexion of Galvanometer with current from 20 Clark Cells sent through the Sulphur.

Deflexion.		Double deflexion.	Zero from deflexion.	Observed zero.
Right.	Left.			
Divs. of scale.	Divisions.	Divisions.	Divisions.	Divisions.
+60	+145	85	+102	+ 95
+50	+145	95	+ 97	+100
+60	+148	88	+104	+100
+70	+150	80	+110	+100
+65	+143	78	+104	+100
+75	+150	75	+112	+ 98
+68	+143	75	+107	+ 98
+70	+135	65	+102	+ 90
+40	+135	95	+ 87	+ 88
+60	+140	80	+100	+ 85
+45	+133	88	+ 89	+ 85
+35	+115	80	+ 75	+ 80

Deflexion of Galvanometer with fraction of current from large Clark Cell.

Resistance taken out of S.	Deflexion.		Double deflexion.	Zero from deflexion.	Observed zero.
	Right.	Left.			
L. ohms.	Divisions of scale.	Divisions of scale.	Divisions of scale.	Divisions of scale.	Divisions of scale.
10	+55	+150	95	+103	+105
9	+60	+143	83	+101	+103
9	+60	+145	85	+101	+103
8	+68	+140	72	+104	+103
8	+70	+140	70	+105	+103
9	+60	+145	85	+101	+100
8	+63	+130	67	+ 96	+ 90
9	+50	+133	83	+ 91	+ 90
9	+45	+130	95	+ 92	+ 85
9	+35	+120	85	+ 77	+ 80
9	+35	+120	85	+ 77	+ 80
8	+40	+115	75	+ 77	+ 78
8	+40	+115	75	+ 77	+ 80

Rejecting those observations with the sulphur in which the zero from the deflexions differs from the observed zero by 10 divisions and over, the mean double deflexion with a current from 20 Clark cells sent through the sulphur is 85·5 divisions, and the mean deflexion with the current from the large Clark cell when $S=9$ ohms is 85·9 divisions.

The difficulties which had to be met during these measurements seemed to arise from what, following Mr. Bosanquet, we at first called "ghosts." These phantoms, however, seem to have arisen from people opening and shutting doors with iron locks; as a general rule the galvanometer was got properly sensitive overnight, and it was found that if on the following morning the light-spot was where it had been left, then the observations were practically successful; if, on the other hand, the light-spot had gone off the scale, then there was not much use in going on. The disturbance of the fibre consequent on restoring the old zero did not seem to wear itself out under at least twelve hours. It is practically impossible to get a galvanometer of this degree of sensitiveness to work with a silk fibre, the zero being always on the move. In order to get rid of air-currents the ventilators of the room required to be covered up, and the well-made galvanometer-case had to be enclosed in a cardboard box. The insulation at first gave great trouble. It is necessary to support the wires on insulating stands of the paraffin-bottle form, the cells, resistance-boxes, galvanometer, &c. on sheets of glass, themselves resting on small cylinders of paraffin. The insulation of the handles of the keys requires attention. Paraffin keys are much better than ebonite ones.

In pushing the sensitiveness of the galvanometer beyond this point, the following precautions should be observed besides those already mentioned. The arrangement for supporting the magnets should be quite independent of the arrangement for supporting the suspension. The base of the instrument should be of gun-metal and all the framework of metal. The adjustments of the controlling magnets must be capable of being made with extraordinary accuracy. The mirror must be good enough to be used in a telescope. The quartz fibre should be at least six feet long; it must be cemented to its connexions with hard paraffin. The whole apparatus should, we think, be placed in a thick soft iron cylinder, but about this we are not sure. We are tolerably certain, however, that it is in anybody's power to construct a galvanometer on these lines with a sensitiveness of 10^{-13}

amperes per scale-division and a time of swing of about 40 seconds. Such an instrument, however, could only be used in a tolerably non-magnetic building, and one steady enough to be free from the vibration caused by people walking about. Our best results were got at night and on Sundays, and this in spite of the room having a concrete floor reposing on twenty feet of broken stone and all the instruments being supported on slate benches.

As to the results quoted, no discussion will be given here as we are investigating pure samples of sulphur. It may, however, be mentioned that the resistance depends considerably on the time the current has been flowing, on the electromotive force, and on the temperature.

XXXVII. *On a Physical Basis for the Theory of Errors.*

By Charles V. Burton, *D.Sc.*[*]

1. In deducing a law of error, two courses seem open to us. We may make our assumptions as general as possible, so that our results shall have the widest application, and shall in the long run approach most nearly to the truth; or we may treat each separate case as a special problem in probability, taking account of all that we know concerning the actual conditions.

I shall here endeavour to illustrate the latter method by means of some examples; proceeding next to the resultant law of error when two or more elements are combined which are independently subject to error. The most advantageous combination of fallible measures will then be shortly discussed, and, finally, subjective or personal errors will be considered.

2. Suppose that we are given a series of numbers, known correctly to any required number of places, and that from this we write down the same series correct to four places. There will be no uncertainty in the operation unless the digit in the 5th place is 5, and all the remaining digits zero; and (in general) the chance of this occurring is indefinitely small. The limits of possible error are obviously ±·00005, and all errors between these limits are equally probable, unless from

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[*] Read November 1, 1889.

our knowledge of the series we have *à priori* evidence to the contrary. The curve of error (as one may call it) is thus a finite straight line A B (fig. 1), parallel to the axis of errors L M, and bisected by the ordinate of no error, O N. If the original table is carried only to (say) 5 places the case will be somewhat changed. About $\frac{1}{10}$ of the series of numbers will have 5 in the 5th place of decimals; the remaining $\frac{9}{10}$ of the series will all be *correct* to 4 places, and will have errors ranging uniformly between ±·000045. Of the first-named $\frac{1}{10}$ of the series, half will have errors between +·000045 and +·000055, and half will have errors between −·000045 and −·000055; the distribution between these limits being uniform*. The corresponding curve of error is given in fig. 2.

Fig. 1.

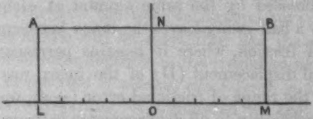
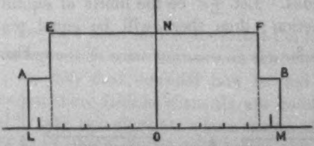


Fig. 2.



3. A similar case is the following. Suppose we have to record successive positions of an index upon a fixed scale, which is graduated in centimetres, and that readings are to be taken to the nearest centimetre. If our judgment were infinitely acute, the errors would lie uniformly between ±·5 centim.; but in practice there will also be subjective errors, the consideration of which is left to a later section (§ 10).

4. Next let us consider the error introduced by friction into the equilibrium position of a movable index. Suppose that the index has one degree of freedom, and that if friction

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* That is, supposing that ·00005 is added in the case of half of these numbers, and subtracted in the case of the remaining half.

were removed its vibrations would be simple harmonic; the frictional coefficient being the same at rest and at all speeds. During a half-swing—say to the right—there will be a constant force (or a constant moment) of friction urging the index to the left, and its motion during the half-swing will be harmonic and in the same half-period as if friction were absent, the only difference being that the mean position of the half-swing lies somewhat more to the left. In the return half-swing there will be the same half-period, the mean position being equally displaced to the right. The amplitude is thus decreased by the same amount at each half-swing, until finally a half-swing leaves the index between the limiting positions of friction, where it remains permanently at rest. If the initial displacement (D) of the index was large compared with the range of frictional error ($\pm d$), we may assign the same probability to all displacements between $D-2d$ and $D+2d$; and since the final displacement differs from D by an exact multiple of $2d$, it immediately follows that all final displacements between $\pm d$ are equally probable, larger errors being impossible. The curve of error will be like fig. 1.

If friction is greater when the index is at rest, the result is rather curious. Let $\pm d'$ be the limits of equilibrium under statical friction; then there will be equal probabilities of errors between the limits $2d-d'$ and $-d'$, and also between $-2d+d'$ and $+d'$. If d' is $<2d$, these ranges of error overlap, and the curve of error is like fig. 3; if d' is $>2d$ the curve is like fig. 4; if $d'=2d$, we have simply fig. 5.

Fig. 3.

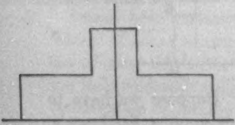


Fig. 4.

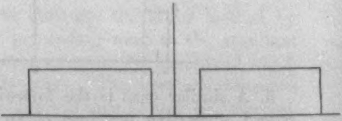
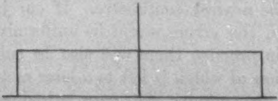


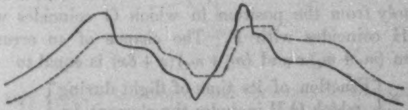
Fig. 5.



These results are easily obtained by considering initial displacements between the limits $4nd + d'$ and $4(n+1)d + d'$, where n is an integer; they refer of course to the actual, not to the observed, position of the index. If, while the index was in motion, three successive excursions were read (with perfect accuracy), the inferred position of equilibrium would only be subject to an error due to *deviations* from the assumed laws of friction.

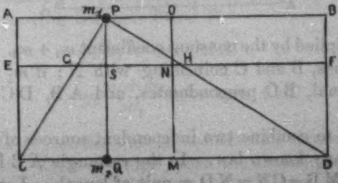
5. Now let a declination-needle which is to trace a continuous record be subject to frictional error. If the black line in fig. 6 represent the true declination-curve, the curve traced by the needle will be something like the dotted line. Here the law of error depends on the (variable) friction of the needle, and *on the kind of changes which occur in the quantity measured*; it will further be influenced by the moment of inertia of the needle.

Fig. 6.



6. Enough has now been said to show that the law of error to be adopted depends in some measure on the nature of each special case; we may next consider how to find the law of error when two or more fallible elements are combined. To commence with, take two elements whose curves of error are of the type of fig. 1, the limits of error being $\pm m_1$ and $\pm m_2$ respectively. In the rectangle A C D B (fig. 7), let A O = O B = C M = M D = the unit of length; and let a particle be chosen whose mass is numerically equal to m_1. If this particle

Fig. 7.



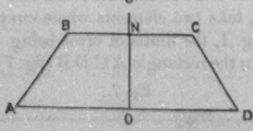
be placed at O P to represent an actual error $= m_1$ O P, it is evident that the *uniform* motion of the particle from A to B represents the distribution of errors between the limits m_1OA and m_1OB, that is, between $\pm m_1$. The second source of error may be similarly represented by a particle of mass m_2 which moves uniformly from C to D. When m_1 is at P and m_2 at Q, the resultant error will be $m_1\mathrm{OP} + m_2\mathrm{MQ} = (m_1 + m_2)\mathrm{NS}$, where S is the centre of mass of m_1 and m_2.

To form the most general series of combinations, let m_2 move backwards and forwards very rapidly between C and D, always with (numerically) the same velocity, while at the same time m_1 moves uniformly and very slowly from A to B. By following the movements of the mass-centre along E F, we shall find the law of frequency of the resultant error. Join P C, P D, cutting E F in G and H; then while m_1 is passing through P, the mass-centre is moving uniformly between G and H; and as m_1 moves from A to B, G H moves uniformly from the position in which G coincides with E, until H coincides with F. The chance of an error lying between $(m_1 + m_2)x$ and $(m_1 + m_2)(x + \delta x)$ is equal to

$$\frac{\delta x}{\mathrm{GH}} \times \begin{bmatrix}\text{Fraction of its time of flight during} \\ \text{which G H includes the element } \delta x.\end{bmatrix} \quad . \quad . \qquad (1)$$

In general the curve of error will take the form A B C D (fig. 8); where A O = O D = unit length, and abscissæ are

Fig. 8.

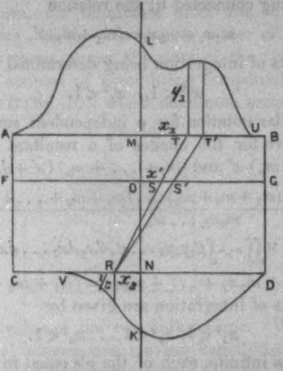


to be multiplied by the constant coefficient $m_1 + m_2$. If $m_1 = m_2$, B C vanishes, B and C coinciding with N; if m_1 and m_2 are very unequal, B C preponderates, and A B, D C are nearly vertical.

7. Next to combine two independent sources of error, each following any known law. In the rectangle A B D C (fig. 9) let A M = M B = C N = N D = unit of length. Let the numerical maxima of the errors be m_1 and m_2, and the curves of

error $y_1=\phi_1(x_1)$ or A L U, and $y_2=\phi_2(x_2)$ or V K D. Following the method of the previous section, let the mass m_1

Fig. 9.



move very rapidly backwards and forwards between A and U or between A and B, having at each point of its path a velocity inversely proportional to the corresponding ordinate; (adding U B to the path makes no difference, since the velocity in this part would be infinite). Similarly let m_2 move from C to D with a very small velocity, which follows a similar rule. First of all let the mass m_2 be passing through R (NR$=x_2$), so that the second error has the value m_2x_2; also in fig. 9 let MT$=x_1$, TT′$=\delta x_1$, OS$=x'$, SS′$=\delta x'$. Then the chance of a resultant error between $(m_1+m_2)x'$ and $(m_1+m_2)(x'+\delta x')$ = the chance that m_1 is moving between T and T′

$$=y_1\delta x_1 \div \text{area ALU};$$

$$=y_1\frac{m_1+m_2}{m_2}\delta x' \div \text{area ALU}.$$

For simplicity, let the scale of ordinates be chosen so that the area ALU = VKD = unity; then the chance that R may lie between x_2 and $x_2+dx_2=y_2\,dx_2$.

Hence, taking both movements into account, the chance of an error between $(m_1+m_2)x'$ and $(m_1+m_2)(x'+dx')$

$$=\frac{m_1+m_2}{m_2}dx'\int y_1 y_2\,dx_2;$$

y_1 and y_2 being connected by the relation

$$m_1x_1+m_2x_2=(m_1+m_2)x',$$

and the limits of integration being determined by

$$x_1^2<1, \quad x_2^2<1.$$

With similar notation for n independent sources of error, we shall have for the chance of a resultant error between $(m_1+\dots+m_n)\,x'$ and $(m_1+\dots+m_n)\,(x'+dx')$

$$dx'\,\frac{(m_1+m_2)(m_1+m_2+m_3)\dots(m_1+m_2+\dots+m_n)}{m_2m_3\dots m_n}$$

$$\times\iint\dots\int y_1y_2y_3\dots y_n\,dx_2dx_3\dots dx_n; \quad . \quad . \quad (2)$$

where $m_1x_1+m_2x_2+\dots+m_nx_n=(m_1+m_2+\dots+m_n)x'$, and the limits of integration are given by

$$x_1^2<1,\ x_2^2<1,\dots x_n^2<1.$$

If we make n infinite, each of the y's equal to $\frac{1}{2}$, and all the m's equal, this reduces to the case from whose approximate solution Laplace deduced his law of error.

8. If we follow up the operation of § 6 by successively adding sources of error of the type of fig. 1, we shall find that the resultant curve rapidly approaches the Laplacian form, the discontinuities being continually of a remoter order. Thus with two errors (fig. 8) the curve meets the axis of errors and has only discontinuities of direction; with three errors there are discontinuities of curvature, and so on. The curve produced by combining n errors of this type will have contacts of the $(n-2)$th order with the axis of errors; and the resultant of two curves with contacts of the mth and nth orders respectively has contacts of the $(n+m+2)$th order; for contacts of the mth and nth orders would be produced by combining $m+2$ and $n+2$ errors respectively.

To illustrate these principles, consider the error introduced into the indications of a balance by the friction of its knife-edges; and to simplify the case, suppose that the balance is allowed to come to rest before a reading is taken. We are only concerned here with errors in the *actual position* of the pointer, supposing the weights correct and the balance just. The friction at each knife-edge is a source of error, and the three sources of error are (at least very approximately) independent. Let the frictional coefficient at each knife-edge be constant, the maximum error due to the central knife-edge being double that due to either of the others; there are then three curves of errors of the type of fig. 1. The two equal sources of error being compounded will give the curve A L B (fig. 10), whose maximum error is equal to the maximum value of the remaining error. We shall thus have two equal masses, one moving rapidly backwards and forwards along C D with numerically constant velocity; the other moving slowly from A to B with a slowness proportional to the corresponding ordinate; and the expression (1) will evidently be applicable. Consider an element δx at H; then the second factor of (1) = chance that the slowly-moving mass lies between P and Q = the area P R L B. As H′ moves from E to F, P Q moves from the position in which Q coincides with A, until P coincides with B. The resultant curve of error will consist of three arcs of equal parabolas (fig. 11) with foci at P, Q, and R. For determinations by the method of double weighing, we must compound this curve with another equal and similar curve.

Fig. 10.

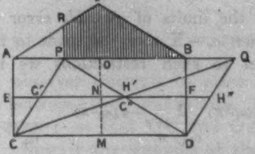
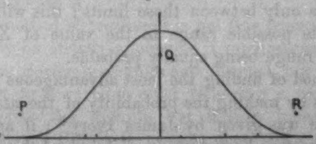


Fig. 11.



9. *Most advantageous combination.*—Suppose we have a number (n) of independent measurements ($a_1, a_2, \ldots a_n$) of the same quantity. Let the chance of an error between x_1 and x_1+dx_1 in a_1 be

$$\frac{1}{c_1\sqrt{\pi}} \cdot e^{-\phi_1(x_1)}\,dx_1, \quad \ldots \ldots \ldots \quad (3)$$

with similar notation for the remaining measures—an assumption which is perfectly general. By adopting X as the *true* value we assume the errors in the respective measures to be

$$(a_1-\mathrm{X}),\ (a_2-\mathrm{X}), \ldots (a_n-\mathrm{X}).$$

If we know the limits of *possible* error for one of the measures, a_r, then $a_r-\mathrm{X}$ must be made to fall within these limits. Subject to such restrictions we must make the product

$$\frac{1}{c_1\sqrt{\pi}} \cdot e^{-\phi_1(a_1-\mathrm{X})} \frac{1}{c_2\sqrt{\pi}} e^{-\phi_2(a_2-\mathrm{X})} \ldots \frac{1}{c_n\sqrt{\pi}} \cdot e^{-\phi_n(a_n-\mathrm{X})}$$

a maximum; that is we must make

$$\phi_1(a_1-\mathrm{X})+\phi_2(a_2-\mathrm{X})+\ldots+\phi_n(a_n-\mathrm{X}) \text{ a minimum} . \quad (4)$$

If Laplace's law of error applies to each of the independent measures, then

$$\phi_1(a_1-\mathrm{X})=\frac{(a_1-\mathrm{X})^2}{\mathrm{C}_1^2}, \ \ldots \phi_n(a_n-\mathrm{X})=\frac{(a_n-\mathrm{X})^2}{\mathrm{C}_n^2};$$

and (4) reduces to the method of least squares. We can also see from (4) how the most advantageous combination of measures depends on the laws of error in the separate observations; and that Laplace's law of error alone leads to the method of least squares.

As another particular case, let each of the quantities $\phi_1(x_1), \phi_2(x_2) \ldots \phi_n(x_n)$ be constant between certain limits, and possible only between those limits; this will in general give a finite possible range to the value of X, all values within this range being equally probable.

The method of finding the most advantageous combination of measures by making the probability of the assumed value a maximum was given by James Ivory*; it appears to me the most direct and satisfactory, and indeed the only method which is applicable in all cases. The intimate relation which has here been shown to exist between Laplace's law of error and the method of least squares is also in agreement with the results somewhat differently deduced in Ivory's paper.

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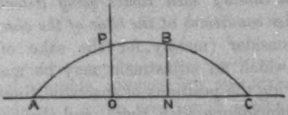
* Phil. Mag. 1825, vol. lxv. pp. 161 *et seq.*

10. *Subjective errors* remain to be considered. Suppose that in § 3, when the index is nearly halfway between two graduations, we may not decide correctly to which graduation it is nearest. Let A B C (fig. 12) represent the curve of error when the observer attempts to place this particular index halfway between two graduations, O P being the ordinate of no error; so that O C and O A represent the limits of error. Now if the distance of the index from a graduation lies between +·5 centim. −OC and −·5 centim. −OA, its position will be taken as certainly nearer to that graduation than to any other, and the corresponding errors for such positions of the index will be uniformly distributed between these limits. Draw any ordinate B N of the curve A B C; then the chance that in trying for the middle of a division we should *discard the position* N *for one more to the right*

Fig. 12.



$$= \text{area } NBC \div \text{area } ABC\,; \quad . \quad . \quad . \quad . \qquad (5)$$

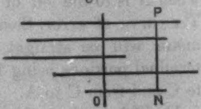
when N is to the left of A, this chance becomes a certainty. The curve of error, then, for readings to the nearest centimetre will be straight and horizontal in the middle, the terminal portions being found by integrating A B C from A to C and from C to A.

11. This method of treatment is not free from assumption; it would only be strictly applicable if our judgment were on each occasion perfectly definite and decided, but at the same time subject to a variable error following a known law of frequency—like the definite but fallible indications of a physical index. But it is evident that when an index is sufficiently near to its true position (or to our more or less biased conception of its true position), we shall have no choice

in saying in which direction the error lies, and therefore no motive in altering the adjustment. Now the bias and want of acuteness of our judgment will of course be variable, the circumstances which affect them (experience, fatigue, illumination, &c.) being almost endless; but I think this principle may be conceded: *If an index be placed in a given position and examined from a given point of view, we shall judge the adjustment to be correct when, but not unless, the error falls between certain limits; such limits being determined by subjective and other conditions at the time of the observation.*

12. Now consider (merely for the sake of illustration) two ways in which an adjustment may be made. Suppose we are measuring the positions of spectrum-lines by bringing a wire into coincidence with them, and disregarding parallactic and instrumental errors, let us confine our attention to errors of judgment in adjusting the cross-wire. First let the determinations be made in this (impracticable) manner: the micrometer-head is first moved at random, and then the field observed, and if, in the judgment of the observer, the vertical wire coincides accurately with a spectrum-line, the micrometer reading is recorded. Whenever an apparent coincidence is registered, the error of judgment must have fallen between certain limits; and obviously it is equally likely to have any value between those limits. In a number of observations the law of error may be determined as follows:—Draw a number of horizontal lines (fig. 13) each representing on a convenient scale the limits of possible error at the time of an observation*. The relative frequency of an error O N will be proportional to the number of horizontal lines cut by the ordinate P N. In general the curve of error for a large number of observations will be straight and horizontal in the middle, falling down towards the ends so as to meet the axis of errors; if the limits of errors of judgment were invariable, the curve of error would be of the type of fig. 1.

Fig. 13.



13. Perhaps in practice we shall more nearly approximate to the following plan:—Starting with the wire in a position

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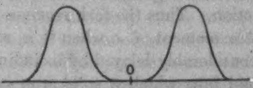
* What we really require to know is the *law of frequency* for the *limits* of error.

which is evidently in error, we shall move it up to the spectrum-line until no further error can be detected. If we really stop moving the wire as soon as we cease to feel a distinct motive for doing so, we shall be just at the limit of possible error corresponding to that time. For a series of *single* observations the curve of error would perhaps be something like fig. 14.

Fig. 14.



14. As a final example, suppose we are trying to place a small round body in a given position in a plane. Here the law of error will depend on our manner of testing the adjustment: whether we merely look up and down and from side to side to get two (very nearly independent) judgments of the corresponding coordinates, or whether we examine the object from every point of view to make sure that no error can be detected. The case may be illustrated mechanically by supposing two pendulums supported one on gimbals, the other after the manner of Foucault's pendulum; appropriate laws of friction being assumed. The law of error will further depend on whether we follow the method of § 12 or of § 13.

If we were trying to drive a billiard ball from one given position so as to make it come to rest in another given position (without rebounds), the errors of direction and of distance would, if small, be very nearly independent; the latter coordinate being probably subject to much greater variation.

We are led, then, to the following conclusions:—The law of error in a set of observations depends on the nature of each special case; and what may be called the probable law of error is determined by our knowledge of the conditions. The combination of three or more sources of error of comparable importance gives in general a law of frequency not seriously differing from that of Laplace; so that the method of least squares will be practically the most advantageous, except where a single source of error with a very different law is predominant above all the rest.

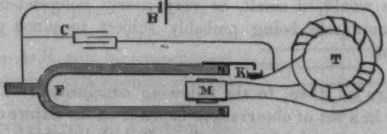
XXXVIII. *On a Method of Driving Tuning-Forks Electrically.* *By* W. G. Gregory, *M.A.*, *Demonstrator in Physics at the Royal Indian Engineering College, Coopers Hill*[*].

In the ordinary method of driving tuning-forks by electricity, the battery circuit is completed just before the end of the stroke and broken again soon after the commencement of the return motion. Thus the fork receives its impulse at a most unfavourable moment, *i. e.* when it is stationary. The impulse can be considerably delayed by including in the battery circuit a solenoid into which a solid iron core is inserted more or less till the best result is obtained. This adjustment, however, varies with the amplitude of vibration and the alterations of the contact. The whole matter was discussed in a paper read before the Physical Society on June 26th, 1886, by Prof. S. P. Thompson[†], who then suggested the employment of two similar forks which drove one another and settled down to a difference of phase of a quarter of a period. Each fork, therefore, received an impulse when moving at its greatest velocity in the middle of its stroke.

The method I now suggest accomplishes the same thing without the employment of a second fork, and moreover gives two impulses in each complete period instead of one.

The arrangement is shown in fig. 1.

Fig. 1.



F is a tuning-fork mounted in the usual way, and provided with a driving electromagnet M, and contact-maker K. But instead of taking the main battery-current directly through the electromagnet M, it is sent through the primary coil of a kind of transformer T, the secondary circuit of which is connected to the electromagnet. Hence, at both make and break of the main circuit, momentary induced currents pass through the driving magnet alternately in opposite directions. By polarizing the fork so as to make it a horseshoe-magnet, the impulses are also made to act alternately as attractions and repulsions, and the fork receives two impulses for each complete vibration. By properly adjusting the contact when the fork is at rest, so as just to complete the circuit, the impulses will be given very approximately at the middle of the stroke, and are independent of the amplitude of vibration. The fork I tried made about 86 vibrations per second, and had prongs 20 cms. long and 1·8 cm. apart. The electromagnet M was formed by winding about 50 turns of no. 22 silk-covered copper wire round a core formed of a bundle of varnished iron wire. It was mounted on a wooden support capable of sliding between and parallel to the prongs. The transformer T consists of a core of cotton-covered iron bonnet-wire wound in the form of an anchor-ring having a mean diameter of 6 cms. and thickness 1 cm. Round this is wound the secondary coil of about 160 turns of no. 22 silk-covered copper wire, forming a single layer completely round, and outside this the primary of about 190 turns of similar wire. A condenser C of 4 microfarads capacity was connected across the mercury-break K to diminish the sparking. One small accumulator B sufficed to work the fork vigorously, the mean current consumed being about 2 amperes. By increasing the number of turns of wire, much less current would be required, but there would be a limit, since the lag must not be so great as to interfere with the proper timing of the impulses.

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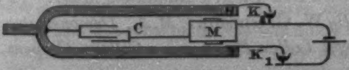
[*] Read November 1, 1889.

[†] Proc. Phys. Soc. viii. pt. ii.; Phil. Mag. [5] xxii. p. 216.

An alternative method, using far less current but requiring two contacts, is shown in fig. 2.

Fig. 2.



The contact K_1 charges the condenser C and K_2 discharges it. The currents of charge and discharge are both taken through the electromagnet M, and the action is the same as above, the fork being, of course, polarized. It would be necessary to wind the magnet M with many turns of wire, to use a battery of high E.M.F. and a large condenser. The latter, however, could probably be replaced by a polarizable battery of platinum or lead wires in dilute sulphuric acid. Except for economy of working, it is not so good a method as the first, as the adjustments of the contacts K_1 and K_2 must be very exact.

As regards magnetizing the fork, there can be no real objection to that, for in the usual method of driving it soon acquires a polarity. In fact the fork I first tried worked at once without any further magnetization.

Prof. S. P. Thompson said the methods, if perfect, would be of great service, and suggested that a fork so driven be tested optically by comparison with a freely vibrating one. He regarded the mercury-contacts used as objectionable, for their capillarity and adhesion would probably cause the impulses to lag behind the appointed epochs.

Prof. McLeod remarked that Lissajou's Figures gave a satisfactory method of testing the constancy of period, and could be readily observed without using lenses; and in reference to liquid condensers suggested by the author for his second device, said that platinum plates in sulphuric acid were found to disintegrate when used for this purpose. He thought lead plates would prove suitable.

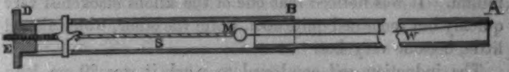
Prof. Jones, who read a paper on a similar subject in March last, said he now used bowed forks with which to synchronize the speed of the disk there described, and the frequency is determined by causing the disk to complete the circuit of his Morse Receiver once each revolution.

XXXIX. *On a New Electric Radiation Meter. (Preliminary Note.) By* W. G. GREGORY, *M.A., Demonstrator in Physics at the Royal Indian Engineering College, Coopers Hill*[*].

THE usual method of detecting electric radiations by observing the sparking across an air-gap in the wire used as a receiver admits of only a rough estimate of their intensity.

[*] Read November 1, 1889.

The object of this paper is to describe an instrument by means of which definite quantitative measurements may be made. It is based on the measurement of the elongation of a wire when heated by the currents induced in it by the rapidly varying field of force.



It consists of a long glass tube A B cemented to a shorter piece of brass tubing B D. Within is stretched a platinum wire W fastened to the glass tube at A and to a Perry magnifying-spring at M, where there is a mirror to indicate the rotation of the spring by the reflexion of a beam of light upon a scale. At E there is a nut and screw for adjusting the tension. The brass tube is partially filed away in front of the mirror to form a window. The object of the brass tube is to compensate the spring for general changes of temperature, the glass tube serving the same purpose for the platinum wire.

The wire employed was ·0086 cm. in diameter, and 192 cms. long. The spring was about 25 cms. long and ·007 cm. in diameter. It was made out of a fine kind of tinsel ·04 cm. wide and ·0015 cm. thick, which can be obtained at fancy shops wound loosely on cotton. When removed from the cotton, it was already in the form of an irregular helix. It was then rolled tightly between the fingers to reduce the diameter as much as possible. A piece of silk was next attached to one end, and the spring extended almost to breaking-point, the silk allowing it freely to untwist as much as it would. After this it was rolled between two hard surfaces and again pulled out. This was repeated till at last 10 revolutions per millimetre extension were obtained.

The mirror was a $\frac{9}{10}$-inch worked concave one, and gave a very sharp image of a wire on a scale one metre off. Using an ordinary galvanometer-scale, it was easy to read to one division. This would correspond to an elongation of the wire of ·000005 of a millimetre, and a rise of temperature about ·003 of a degree Centigrade. In spite of this sensitiveness, only one or two divisions' deflexion were obtained with the oscillator 4 metres off. The oscillator used consisted of two brass rods ·53 cm. in diameter supported horizontally and carrying two zinc plates 40 cm. square, capable of sliding along them so that the wave-length could be altered at pleasure. They were usually kept about 25 cm. apart. The terminal knobs were 2 cm. in diameter and the spark-gap about 2 or 3 millim. It was noticed that one of the knobs blackened very quickly but remained quite cool, while the other, which altered little, became very hot.

The induction-coil employed to work it was 20 cm. long and 12 cm. diameter, and gave with the battery-power employed sparks 4 cm. long between two points. To obtain greater regularity in the working of the coil, a tuning-fork mercury break, vibrating 86 times a second, was used.

By employing a larger coil larger deflexions could, no doubt, be obtained and effects at greater distances observed.

A method was tried for increasing the sensitiveness by weighting the mirror so that its centre of gravity was behind and rather above the axis of suspension. The tension was adjusted so that the spring kept the mirror near its position of unstable equilibrium. The effect was to render the smaller deflexions nearly 10 times as great, while the sensitiveness diminished as the deflexions became larger. But the difficulties of working with it were greatly increased, as the smallest draught of air would alter the zero-point.

Wires of different materials were also tried which should theoretically give better results; but, probably owing to their diameters not being sufficiently fine for the tension to which they were subjected, the results were not satisfactory. The convenience of being able to compensate the platinum wire by the glass tube seems, however, a great point in favour of the use of platinum, and has led me hitherto to keep to it.

Prof. Perry asked if the E.M.F. required to produce the observed results had been calculated; he also believed that the sensibility might be increased by using copper instead of platinum, and replacing the spring by a twisted strip.

Mr. Blakesley inquired whether the effect of increasing the capacity of the ends of the wire had been tried.

Mr. Boys said that if the observed effect was due to rise of temperature he would like to see it measured thermally. He also thought the effect might be due to extension caused by rapid electric oscillations in some such way as the elongation of an iron bar caused by magnetization. In answer to this, Prof. S. P. Thompson said the matter had been investigated experimentally, but with negative results.

Prof. Herschel suggested the use of a compound spring such as are used in Bregnet's Metallic Thermometers.

In reply Mr. Gregory said he had estimated the E.M.F. by observing that a Leclanché cell through 50 ohms produced about the same result. No improvement in sensitiveness was obtained by using copper wire or by increasing its capacity, and attempts to measure the rise of temperature by an air-thermometer had been given up as hopeless.

The President, in thanking the author for his paper, congratulated him on the ingenuity and courage displayed in producing an apparatus to measure such microscopic quantities as are here involved.

XL. *On Electrifications due to the Contact of Gases with Liquids.* *By* J. ENRIGHT, *B.Sc.*[*]

A STUDY of Helmholtz's theory of atomic charges led to the experiments I am about to describe. When one has got the idea that molecules are kept in their integrity by the mutual attractions of the oppositely charged atoms or radicals composing them, a question soon arises as to what part, if any, these charges play in ordinary chemical reactions. The theory precludes any other form of chemical affinity than that due to the mutual attractions of the opposite charges, and it occurred to me that since at the moment of reaction there must be disturbance and re-arrangement of them, some indication of their existence might be obtained by allowing reactions to go on in an insulated vessel properly connected with a quadrant-electrometer. Even if a permanent electrification could not follow, I thought it just possible that a momentary flutter of the "spot" might take place on account of the electric disturbance due to the interchange of the charged ions.

[*] Read November 15, 1889.

This consideration alone was sufficient to excite one to experiment, but if I tell the whole story I must say that I had had other expectations. According to theory the charges on monads are exactly equal. How then does it happen, if reactions be a result of the charges, that chlorine drives iodine out of potassic iodide? How, indeed, does it happen that any one molecule composed of a particular pair of monad elements is stronger,—that is to say, more stable than any other molecule composed of any other pair of monads? And so on. After pondering on such questions for some time, I could not suppress the thought that after all perhaps the charges might not be equal, and that the experiments I had determined to carry out might possibly throw some light on the matter.

Then there were certain special reactions which appeared to afford an admirable opportunity for experimental study of the theory. I refer to cases in which the atomicity of elements undergoes a change during a reaction,

$$2Fe^{II}SO_4 + H_2SO_4 + Cl_2 = Fe_2^{IV}(SO_4)_3 + 2HCl \text{ (Roscoe).}$$

Here four monad charges of positive electricity attached to the iron on the left change into eight monad charges on the same quantity of iron on the right. If the increment of positive electricity be produced by contact an equal charge of negative ought to be set free, and this ought to be easily detected by the electrometer.

On the whole there appeared, to a strong believer in the theory, a fair and inviting field for experiment. My plan was to examine in the manner indicated instances of the four following types of chemical change :—

(1) Combination of elements.
(2) Displacement.
(3) Double decomposition.
(4) Change of valency.

I may as well say at once that from (3) and (4) I got no result whatever, from (1) only an electrification of little importance, and that it was from (2)—instances of displacement—that the results I wish to call attention to were obtained.

The electrometer was made rather sensitive. A high-resistance Daniell through which a current never passed gave a deflexion of 94 divisions either way. An insulated plate on which the reactions were to proceed was fixed so that it could be connected or disconnected at pleasure with the insulated pair of quadrants. The insulation of this plate was tested by giving to it a small charge, and connecting it then with the quadrants. The spot moved and came to rest 200 divisions from zero. It was watched for 3 minutes, during which it maintained its position.

A porcelain dish containing about 20 c. c. of a strong solution of potassic iodide was placed on the insulated plate. About the same volume of a strong solution of chlorine in water was placed in a beaker to which was attached a handle of insulating material, and by means of this handle the chlorine water was poured into the potassic iodide. A heavy brown precipitate appeared. The insulated plate was now connected to the insulated quadrants. No deflexion resulted. The experiment was repeated without producing a movement of the spot.

The porcelain dish was now half filled with distilled water, placed on the insulated stand, and a fragment of potassium cast into it. The usual reaction ensued and the "spot" moved 160 divisions to the left, came to rest, and maintained its position. The quadrants were short-circuited, and the spot returned to within 2 divisions of zero.

At first view this appeared exactly what I had expected to occur. On reflection, however, many possibilities suggested themselves and each had to be considered. Had I merely detected electricity in the air? May not the deflexion be due to the inductive action of the charged needle on the quadrants? Was the escaping hydrogen electrified, and if so what was the sign of the charge? I proceeded to repeat the experiment.

The quadrants were short-circuited, and the insulated dish, after being momentarily put to earth, was, as before, connected to the insulated quadrants. A second fragment of potassium was tossed into the water in the dish. The spot moved 28 divisions to the left, then turned and moved up the scale to the right, coming to rest at 200 divisions from zero. The quadrants were once more short-circuited, and the experiment again repeated. The spot moved 40 divisions to the right. I could not possibly explain such irregular behaviour. I carefully looked up the connexions and tested the instrument. It was in good order. I again repeated, but with similar results.

Sodium was used instead of potassium, and although the deflexions with it were also irregular, the tabulated results showed a contrast. When sodium was used, 40 per cent. of the deflexions were first to the left; when potassium was used, 70 per cent. were first to the left. Speaking broadly, this seemed to indicate that when potassium was used the dish received a negative charge, when sodium was used the dish took a positive charge; and as I fancied that such a result would enable me to set up some theory with regard to the atomic charges, I tried very hard to eliminate what I then believed to be accidental exceptions and to prove that in reality such was the case.

The porcelain dish was replaced (1) by a glass beaker, and (2) by a metallic dish, the experiment being repeated in every other particular, but without obtaining greater regularity in the deflexions.

I wish to remark here that the method of testing the electrification by keeping the insulated vessel from which gas is escaping in connexion with the insulated quadrants during the reaction is faulty, inasmuch as that under the circumstances a deflexion may be caused by the inductive action of the charged needle on the quadrants. It has, however, this great advantage, viz., that one can see distinctly every variation that may take place during any interval. It will be noticed that throughout the inquiry I use this method for preliminary testing, and that I conclude nothing until I verify by the more rigid process of allowing the reaction to go on while the insulated plate is disconnected from the quadrants, and testing the electrification at its close.

Having failed to get a definite and constant result with water, I proceeded to try other liquids. Sodium and dilute sulphuric acid, sodium and nitric acid, and many other such combinations were tried, and eventually I found that sodium and strong acetic acid gave an *invariable* result, viz., a positive charge on the dish in which the reaction took place. This has not failed hitherto in a single instance.

I reviewed the state of affairs. The first thing to do, as it appeared to me, was to ascertain whether or not the hydrogen carried a charge, and, if so, to determine its sign. I found it so difficult to work with sodium that after many unsatisfactory attempts I decided to search for another metal which would give a similar constant and definite result. One naturally turned to zinc.

A piece of zinc was thrown into dilute sulphuric acid which was put into the insulated dish, the connexions being as already described. The spot rapidly moved 36 divisions to the left, turned, went back across zero and up on the other side 120 divisions. This was not encouraging. It seemed to be the beginning of another set of irregular deflexions. It, however, went to show that electrification was an accompaniment of most chemical reactions. I repeated the experiment several times, varying the degree of dilution of the acid, but failed *then* to get constant results. I next tried hydrochloric acid.

A small portion was placed in the insulated dish and a particle of zinc thrown into it. The spot rapidly moved off the scale to the left, indicating a negative charge on the dish. The quadrants were short-circuited, and the spot returned to zero. Repetition was followed by the same result, and fortunately this turned out to be as constant a deflexion to the left as that from sodium and acetic acid was to the right, while it was much greater in quantity.

I now concentrated my attention on zinc and hydrochloric acid. In this experiment, as in all others where a gas escaped into the air, it was possible, though not likely, that the resulting deflexions were due to atmospheric electricity, and I thought it as well to settle this point at once. A piece of sponge was saturated with alcohol and placed on the insulated plates. It was then fired, and burned with a large flame. The insulated plate was connected to the quadrants when the sponge ceased to burn, and the spot at once moved 10 divisions to the right, indicating a small positive charge on the plate. The experiment was repeated in every particular except the burning of the sponge, but with the result of only a disturbance of the spot of a few divisions.

The air in the neighbourhood of the insulated plate appeared to be slightly electrified, but as it was of opposite sign to the charge produced by the action of zinc and hydrochloric acid, it was certain that the latter was not due to any atmospheric cause. But a new point of great importance had arisen. Whence came the positive charge in the air near the instrument? The idea of its being due to the evolved hydrogen at once suggested itself. For some time before testing with the sponge, I had been repeating the experiment with zinc and hydrochloric acid in order to satisfy myself as to its constancy, and a considerable quantity of hydrogen had escaped into the air.

The charge on the evolved hydrogen opposite in sign to that on the insulated generator! If this were really the case I saw that I should have to give up certain ideas which I had been fostering while dealing with these experiments. The point had to be at once cleared up.

A zinc goblet was metallically attached, mouth downwards, to the insulated plate. A long-necked flask containing zinc and hydrochloric acid was placed underneath in such position that the hydrogen found its way into the inverted goblet. In two minutes after so placing the hydrogen generator the goblet was tested by connecting it with the insulated quadrants. The spot moved 78 divisions to the right, indicating that the hydrogen escaped with a positive charge—that is to say, as already suspected, its charge was opposite in sign to the acid and generator from which it was evolved.

The experiment was several times repeated, and always with the same result, but it was noticeable that the positive charge on the hydrogen was much less in quantity than the negative charge on the generator. If they were in reality oppositely charged, the quantities, as in all such cases, would be exactly equal. I urged, in explanation, that the hydrogen moving over the moist glass of the long-necked generator must lose some of its charge. However, a confirmatory experiment was devised.

A second insulated plate was set up and connected metallically with the first. A hydrogen generator, containing zinc and hydrochloric acid, was placed on one, and a suitable pneumatic trough was placed on the other, in such manner that the gas might be generated and collected on the insulated plates, which were connected to the insulated quadrants. A stream of hydrogen was allowed to pass, and by means of an insulating rod the delivery-tube could be placed so that the hydrogen might get into the air or be collected and rétained on the insulated plates at pleasure. It was found that as long as the gas did not escape the spot remained motionless, but that when it escaped from the plates into the air the spot moved. This, I took it, proved that the charges on the generator and evolved gas were equal in quantity, though opposite in sign.

The state of the case was now, so far, pretty clear. When hydrogen was produced by zinc and hydrochloric acid the hydrogen became positively, the generator negatively, charged. I set this fact up before my mind and examined it as carefully as I could on every side. I turned it round and round, looking for some information as to the atomic charges, but I could find none. Had the charge on the escaping gas been of the same sign as that on the generator, I could have set up some hypothesis concerning them, but, as things were, nothing of the kind was possible.

But what is the cause of this undoubted electrification? I repeated the experiment many times. I varied the quantities of acid and zinc, and noticed that with weaker acid or a larger quantity of zinc the deflexions were not so strong. During these repetitions I was in the habit of short-circuiting the quadrants when the spot came to the end of the scale, lest the suspending fibres of the needle should get a *set* from being twisted too much in one direction. On one occasion, however, being called away suddenly, I neglected to do so, and the electrification worked its will, so to speak, on the needle. On returning in about twenty minutes, I found that all the zinc was dissolved, and to my very great astonishment the spot was at rest at the extreme right of the scale.

Up to that time the deflexion from zinc and hydrochloric acid had been to the left; and, as I had made the experiment a great many times with this unvarying result, I was much perplexed. The uncertain behaviour of sodium and potassium was still fresh in my memory.

However, what to do on the occasion was quite clear: I repeated the experiment in every particular, sat down, and watched it. The spot went rapidly off the scale to the left as usual. The reaction proceeded. In eleven minutes the spot appeared again on the scale, went down 100 divisions rather slowly, then more rapidly, crossed the zero, and at a good pace went up the other side almost to the end of the scale. This was quite a new development. Frequent repetition, with the same result, proved that this behaviour, strange as it was, had a definite cause. I varied the strength of the acid and the quantity of zinc, and found that by either making the acid weak or increasing the quantity of zinc I could hasten the reversal of the sign of the electrification of the generator.

I now concentrated my attention on this reversal of sign. I contrasted all the circumstances attending the deflexion to the left with all those attending the deflexion to the right, and could detect only one difference, and that was the production of chloride of zinc in the generator, and I could not fail to see that as the quantity of this salt increased the deflexions to the right became stronger.

From this change of sign of the generator, when a certain quantity of chloride of zinc got into solution, there suddenly appeared to come a ray of light which I thought dispelled the fog which hung round the earlier experiments. The nature of the liquid through which the hydrogen passed apparently determined the sign of the electrification. I paused again to review the events of my little enterprise.

I set out in quest of some direct evidence as to the atomic charges. I had tried the four types of chemical change. From double decomposition and from cases of change of valency I had got no result whatever. From direct union of elements—from the combination of phosphorus and iodine, accompanied by a cloud of P_2O_5, and, I suppose, iodine vapour —I had got an electrification. From many cases of displacement I had got considerable electrifications; but from some, such as the displacement of iodine by chlorine, I had got nothing whatever. A careful examination of these results disclosed the fact that no electrification was obtained except when something escaped from the vessel in which the reaction occurred. This circumstance I thought could not fail to have some meaning. Then there were the leading incidents, as I considered, of the inquiry:—(1) The escaping gases or vapours came off with a charge opposite in sign but equal in quantity to that on the generator. (2) The reversal of the sign of both generator and escaping gas when the liquid in the generator underwent a definite change.

Such were the facts to be brought under some theory. I was reluctantly compelled to conclude that they had nothing to do with atomic charges, but for all that they afforded food for the most delightful contemplations, often for hours at a stretch, during the last two or three years.

Reflect as I may on the reversal of sign of the electrification when a certain quantity of chloride of zinc gets into solution, I can find no explanation for it but the electricity of contact. Hydrogen is positive to hydrochloric acid, but negative to chloride of zinc. And, indeed, looking at the whole matter in this light, from my present standpoint, I can perceive how, in the earlier experiments, the nature of the liquid through which the hydrogen passed decided the sign of the electrification. From hydrochloric acid it was positive; from acetic acid (or it may be acetate of sodium) it was negative. Even the very whimsical deflexions from sodium and water become intelligible. As a piece of sodium careers about on the surface of water, the escaping hydrogen is one instant in contact with water, the next with caustic soda, the next with the side of the vessel, and, accordingly, on the theory of contact, the irregular deflexions are at once explained.

In most of the experiments described above, the hydrogen was allowed to escape from an open dish or beaker, and not from a narrow delivery-tube, for it was found that the gas partially discharged itself when it passed through narrow openings. Whatever tended to electrically connect the charged liquid with the oppositely charged escaping gas, also tended to lessen the charges on both. And besides this, there was the possibility that the contact of the gas with the delivery-tube might cause electrification. On the whole, it was found least ambiguous to half fill an evaporating-dish about 6 inches in diameter and 2 inches high with HCl and drop into it a few fragments of zinc. The bubbles of gas then shot straight up through the middle of the liquid, and passed into the air without coming into contact with anything else. It was, however, a defect of this mode of proceeding that particles of the acid were projected about in all directions, sometimes beyond the dish. In order to get rid of any doubts on this head, a smaller dish was used and placed at the bottom of a beaker 7 inches high. I propose to give in detail one of several experiments made in this way with all possible care.

The electrometer was *not* in a very sensitive state. The high-resistance Daniell gave a deflexion of 38 divisions on either side. A glass beaker 7 inches high and 5 inches in diameter was placed on the insulated plate. A porcelain dish, 2¾ inches in diameter and 1⅛ inch high, was nearly filled with a 10-per-cent. solution of HCl in distilled water, and placed at the bottom of the beaker just mentioned. Three small fragments of zinc were now dropped into the dilute acid in the small dish. A very slight effervescence at once appeared, and it gradually increased, but never became violent. No trace of spray could be detected at the end of the experiment above the lower half of the beaker. In 4 minutes from dropping the zinc, the spot could be perceived moving, and in 4½ minutes more it had moved 28 divisions to the left, indicating the charge on the dish negative. I append the notes taken during the time the experiment lasted

	Time.		Reading.
	h	m	
Zinc dropped into acid. Insulated quads connected to insulated generator	2	10	378 (zero)
Spot moving to left. Generator positive	2	14	377
Spot moving faster	2	16	367
	2	17	360
Insulated quads and insulated generator disconnected	2	18½	349
	2	19½	349
Insulated quads and generator reconnected	2	20	329
	2	21	322
Spot stops and appears to return	2	21¼	319
	2	23	328
	2	25	346
	2	26	366
Spot crossed zero	2	26¼	378
	2	28	388
Quads and generator disconnected	2	31½	458
	2	33	458
Quads and generator reconnected	2	33¼	513
Effervescence gets less	2	35	550
	2	37	562
	2	40	568
Quads short-circuited	2	42	382 (zero)

It may yet be urged that when hydrogen passes through hydrochloric acid, it carries with it both HCl gas and vapour of water, and that the electrification is due to these, and not to the hydrogen. In order to test the suggestion, I placed on the insulated plate a dish nearly filled with boiling concentrated HCl. In two minutes the plate was connected with the insulated quads, but the spot moved only 1 or 2 divisions. It was then watched for more than 5 minutes, but the spot did not stir although clouds of vapour were escaping from the acid.

I cannot help remarking on the difficulty of neutralizing a gas—at all events, hydrogen. The following experiment is well calculated to illustrate this properly, while it affords, I submit, a very stringent verification of the experiment to which the notes are appended. The large beaker with the small dish was set up in the manner described in the experiment just referred to. The dish was nearly filled with a 16-per-cent. solution of HCl in distilled water. Three small fragments of zinc were thrown into it. A disk of sheet-zinc with a circle one inch in diameter, cut from its centre, was placed as a cover on the beaker. Over the aperture in the centre were placed, one above the other, 4 flat pieces of perforated zinc, the perforations being $\frac{1}{16}$ of an inch in diameter. The hydrogen had to fight its way through this barrier, and yet it carried with it the greater part of its charge, as the following notes of the experiment prove. The high-resistance Daniell gave a deflexion of 72 divisions either way.

	Time.		Reading.
	h	m	
Zinc dropped into acid. Cover placed on beaker. 4 flat zincs placed over aperture	3	23	365
	3	25	364
	3	27	363
	3	28	358
	3	29	343
Spot comes to rest and returns	3	30	333
	3	31	333
	3	32	343
	3	33	353
	3	34	363
	3	35	373
	3	36	383
	3	37	383
	3	39	413
Effervescence ceasing	3	42	413
Quadrants short-circuited	3	43	368

On examining the perforated zincs used in this experiment, each was found to have either spray or condensed vapour deposited on it, but the quantity on the lowest one was greater than that on any of the others. I was not, therefore, clear as to what part the spray played in the matter, and I set myself to consider what experiments I could make to clear up the point. Eventually I decided to make the following two, which I submit as decisive:—

(*a*) Every one has noticed that when hydrogen escapes from a generator containing acid and zinc, particles of the former are projected with considerable force from the vessel. I easily succeeded in catching these particles, or some part of them, on the metallic part of a proof-plane. I then touched the electrode of the insulated quads with the proof-plane, and found that the charge produced a deflexion to the left, indicating that the spray was of the same sign as the generator, and consequently opposite in sign to the charge carried by the gas. It also appears from this experiment that electrical repulsion among the particles themselves constitutes part of the force which causes their flight through the air.

(*b*) The bottom of a metallic goblet was perforated with holes about $\frac{1}{8}$ inch in diameter. It was then attached, mouth downwards, to the electrode of the insulated quadrants. A hydrogen-generator containing zinc and HCl was placed under it in such a position that the evolved gas passed right through the goblet. Almost instantly the spot moved rapidly to the right, which I had expected. The generator was now withdrawn. The spot slowly returned to within 5 divisions of zero.

Now if the electrification were due to spray, the goblet would retain its charge (the insulation was good); but being due to the hydrogen, which gradually passed through the perforations into the air, it very rapidly lost it.

In the course of these experiments I had often cast about for means of measuring the difference of potential arising from the contact of zinc and hydrochloric acid, and on consideration it appeared to me that this experiment presented a possible method. Particles of matter charged with electricity, and impinging on an insulated conductor, to which they give up part or the whole of their charges, could not raise the conductor to a higher potential than they had attained; but they, in the course of time, ought to bring it up to that level. My notion was to allow the charged hydrogen to flow through the goblet, fixed as described, long enough to ensure its reaching the potential of the gas; then to remove the generator, and allow the gas to pass away through the perforations. The permanent deflexion then obtained ought, I considered, to be the measure of the quantity in question. I carried out with all the care possible several experiments on these lines; but on account of the tenacity with which the gas holds its charge, I failed to get satisfactory determinations.

At this stage of my inquiry, I felt that I had got a few definite facts, that I had cleared up, at all events to my own satisfaction, many points which were at first doubtful, and that the theory of contact which I proposed was consistent with all the observations I had made. I proceeded to devise new experiments with the view of testing the truth, and determining the scope, of this theory.

Hydrogen passing through HCl and H_2SO_4 takes a positive charge; passing through the corresponding salts of zinc it takes a negative charge. Then at any moment it ought to be possible to reverse the electrification by introducing into the vessel in which the reaction is going on acid or salt, as the case may be. The old experiment was made. Hydrogen was escaping from a 16-per-cent. solution of HCl, and the spot had travelled 100 divisions when a strong solution of zinc chloride was added. The spot went quickly back and up on the other side of the scale, showing that reversal had taken place. The addition of HCl produced a second reversal.

I had found hydrogen positive to two acids, and a question as to whether this was general often floated before me. I had no means of testing with nitric acid, because its action on metals does not give rise to hydrogen—at all events not in the final stage. I therefore directed attention to the other side of the question. Hydrogen was proved to be negative to chloride and sulphate of zinc. Was it possible that it was negative to other salts, or to salts in general?

I again made the old experiment with Zn + HCl, and when a deflexion of 50 divisions was obtained a strong solution of ferric chloride was added, with the effect of reversing the sign of the electrification. I proceeded to try other salts, such as $NaCl$, $AmCl$, $FeSO_4$, $MgSO_4$, $CaCl_2$, and found that these also produced reversal. I found, however, a few salts which did not do so, notably $Co(NO_3)_2$.

A wide principle thus appeared to be foreshadowed, viz., that hydrogen is positive to acids but negative to salts; but the number of cases examined does not warrant one in laying it down absolutely. Moreover, I have found that the hydrogen passing from acetic acid, whether by the use of sodium or iron filings, always comes off with a negative charge. I strongly suspect, however, that the reversal of sign in these cases takes place so rapidly that it escapes observation. I have had several instances of the spot moving a small distance to the left, suddenly turning, and moving far up on the opposite side of the scale. Occurrences of this kind leave no doubt on my mind that the first electrification (that due to the gas passing through the acid) is often small, owing, I think, to the rapidity with which the salt produced in the reaction diffuses.

Is hydrogen among gases unique in becoming charged when it passes through acids? was the question that next presented itself. I considered what reactions were likely to give any information with regard to the matter, and made numbers of experiments in the manner already fully described. I found that as a rule electrification results from any reaction giving rise to an escape of any substance into the air. In many cases the electrification is very trifling, and the electrometer must be in a sensitive state in order to show it. Of all the reacting combinations which I have tried, $HCl + Zn$ give by far the greatest difference of potential between generator and expelled gas. On the other hand, from $H_2SO_4 + NaCl$ I failed to get any trace of electrification. I give in the following Table *only* those reactions which by frequent repetition I have found to give constant results. I also give the estimated difference of potential, but shall postpone to a later stage a description of the method of making the measurement.

Reacting substances.	Escaping gas.	Gas passing		Quantity D. P. in volts.
		Through acid.	Through salt solution.	
1. $HCl+Zn$	H	+	–	42
2. $HCl+CaCO_3$	CO_2	+	–	
3. $H_2SO_4+Na_2CO_3$	CO_2	+	–	9
4. $H_2SO_4+Na_2SO_3$	SO_2	+	–	
5. $HCl+Na_2SO_3$	SO_2	+	–	
6. H_2SO_4+Zn (50°/$_0$ acid)	H	+	–	16
7. $HCl+FeS$	SH_2	+	–	
8. H_2SO_4+FeS	SH_2	+	–	
9. HCl+Bleaching-powder	Cl	+	–	
10. H_2SO_4+Bleach.-powder	Cl	+	Could not get this to reverse.	
11. HNO_3+Bleach.-powder	Cl	Could not get the usual positive deflexion.	–	
12. HNO_3+CaCO_3	CO_2	Could not get positive deflexion.	–	
13. $\left\{\begin{matrix}CH_3\\COHO\end{matrix}\right.+Na$	H	Could not in either case get the normal positive deflexion.	–	
14. $\left\{\begin{matrix}CH_3\\COHO\end{matrix}\right.+Fe$	H		–	
15. $H_2SO_4+CaCO_3$	CO_2	+	Could not get the reversal.	
16. H_2SO_4+NaCl	HCl	Nil	Nil	

It will be seen that there are nine instances in which the electrifications occur normally—that is to say, in which the charge is of one sign when the ejected gas passes through acid, but of the opposite sign when the corresponding salt gets into solution. I wish to direct special attention to the instances of abnormal behaviour, which I believe to be entirely due to the solubility of the salts produced in the reaction. When these get rapidly into solution and diffuse through the acid, the electrification due to the gas passing through acid cannot be obtained, There are two cases with HNO_3 and two with acetic acid where this happens, and we know that the salts derived from these acids are very soluble. On the other hand, there are two cases where H_2SO_4 takes part in the reaction, in neither of which could the electrification due to the passage of the gas through the solution of salt be obtained. In both instances sulphate of calcium is formed, and we know that this is rather insoluble. The ejected gas therefore never passed through a solution of salt. My contention here derives great support from the fact that when Na_2CO_3 is used with H_2SO_4 to give CO_2, both deflexions are obtained. The only difference between the two reactions is that Na_2SO_4 is very soluble compared with $CaSO_4$.

Taking a broad view of these experiments, they appeared to me to indicate that, in general, gases were positive to acids, but negative to solutions of salts. I could not, however, throw off a feeling of dissatisfaction which arose from my not being able to suggest a trace of any *à priori* reason for such a thing. I fancied I saw some reason why hydrogen should be positive to acids and negative to salts; but that the generalization should be so wide as to extend to all gases, seemed to me most improbable. I felt disposed to question my theory of contact altogether, and resolved to make experiments of a different kind for the purpose of testing it.

If hydrogen be positive to hydrochloric acid, then, why not, I asked, prove it by a straightforward experiment such as passing a stream of the gas through the strong acid? Two insulated plates were set up near the electrometer and connected by a piece of copper wire. A beaker 2 inches in diameter and 5 inches high containing strong HCl was placed on one, and a hydrogen generator furnished with a narrow delivery-tube more than 12 inches long was set on the other. The delivery-tube was suitably fixed, and a brisk current of hydrogen passed through the strong hydrochloric acid. The hydrogen was found to come off with a positive charge, but on allowing it to get into the air direct from the delivery-tube it also came off charged in the same way. Here, then, I was balked. Of course the object of the long delivery-tube was to neutralize the gas before it touched the strong acid in the beaker. I have often remarked how firmly the hydrogen held its charge, but I was quite unprepared to find that it passed through more than 12 inches of wet glass tubing $\frac{1}{8}$ of an inch in diameter and escaped strongly electrified. I next passed the hydrogen through a wash-bottle, but it again came off charged. I passed it through all manner of bent tubing, made all possible shifts in fact to de-electrify the gas in order to fairly carry out my experiment, but without success. In sheer desperation I snatched up a litre-flask, filled it with water, and displaced the latter at the pneumatic trough by hydrogen from Zn + HCl. I then placed the flask mouth upwards and open on the insulated plate which was connected to the quadrants. The spot gradually moved to the left, and came to rest at a distance of 180 divisions from zero.

This experiment yielded more food for thought. At first sight the deflexion appeared to be in the wrong direction, and I had to consider whether the hydrogen could, by any possible process, be neutralized.

I refilled the flask with hydrogen from zinc and hydrochloric acid and corked it before lifting its mouth from under the water, set it on the table, left it for 1¾ hours, then uncorked it and placed it on the insulated plate. A deflexion to the left of 100 divisions was gradually produced as the hydrogen left the flask. By repeated trials I found that it required from four to six hours to neutralize a flask or even a metallic vessel filled with hydrogen from zinc and hydrochloric acid.

But what of the deflexion in the wrong direction? On reflection, the explanation of the deflexion being to the left—that is to say, the insulated quadrants being negatively charged—very quickly appeared. The flask was held in the hand while being filled with hydrogen holding a positive charge. This acted inductively, repelling an equal quantity of positive to earth through my hand and body, and binding on the outside of the moist flask a charge of negative. When the flask was set on the insulated plate the hydrogen, owing to its lightness, gradually escaped into the air carrying its charge with it; and for every portion of the positively charged gas which left the flask a corresponding part of the negative charge bound on its wet outside was set free and spread over the quadrants, causing the deflexion. In fact the wet flask containing the charged hydrogen constituted a veritable Leyden jar.

I was anxious to verify this view. A hydrogen generator with a long neck and containing Zn + HCl was placed on the table. A large wide-mouthed wet flask was attached to an insulating handle, and, by means of this handle, was held mouth downwards in such position that the hydrogen from the long-necked generator filled it by displacing the air. It was then taken by means of the insulating handle *near* the insulated plate (which was connected to the insulated quadrants), when the spot moved along the scale to the right, indicating that the quadrants were positively charged by the inductive action of the charged gas. The flask was next touched on the outside by the finger and again held *near* the insulated plate, but the spot did not move. This was quite in accordance with the Leyden-jar view, for the bound charge on the outside of the flask could have no effect on the insulated plate or the quadrants connected with it. The flask was now placed on the insulated plate, mouth upwards, and as the hydrogen escaped into the air the spot moved up the scale to the left, indicating that the bound negative charge on the outside of the flask was being liberated and charging the quadrants.

This afforded an instance of a kind of distribution of static electricity rather novel. I could only liken it to ground sulphur electrified by the grinding. Most instances of charged bodies are connected with surfaces, but here appeared a case where electricity pervaded an enclosed space. The only way in which I could figure it in my mind in connexion with the hydrogen was by furnishing each molecule of the gas with a charge with which it could not part until, in the ordinary movements of the molecules (according to the dynamical theory of gases), each came into actual contact with the moist inner surface of the flask containing the gas. This way of looking at it enables one to understand the slowness with which the electrified hydrogen becomes neutral; and as the time required to neutralize a vessel of gas of given shape and dimensions must be a function of the average velocity of its molecules at the existing pressure, suitable experiments may, possibly, lead to a direct determination of this quantity.

Musing over these experiments, the wet flask holding the charged hydrogen appeared, to me, not to differ materially from a soap-bubble blown with hydrogen from Zn + HCl. If there be anything in the notion, such a bubble should constitute a charged condenser with the binding charge completely enclosed. During the process of blowing it the repelled charge would pass to earth through the wet delivery-tube and generator. Figuring such a condenser floating through the air, I speculated as to how it would behave itself. As long as it remained in its integrity, no manifestation of its electrical condition could be given. Should the film, however, get fractured its nature would display itself. The gas containing the binding charge would instantly pass away, leaving the bound charge free on the water composing the shell, and it would pass to earth at the first opportunity.

Accounts have been given from time to time of curious bodies which make their way into dwelling-houses during periods of electrical disturbance in the air. They are described as gliding about, apparently examining nooks and corners, and often going up the chimney exploding (as it is called) on the way. Now if we consider a portion of electrified air enclosed by a film of some substance such as water, it would in most respects behave like a soap-bubble blown with hydrogen from zinc and hydrochloric acid. Being of nearly the same density as air, it would not rapidly ascend but move about, impelled by every slight draught. It would be a charged condenser, and the moment any rough or sharp prominence fractured the film, the bound charge would pass to earth, doing mischief in its track. Such an enclosure of electrified air might possibly be effected by an abnormal distribution of heat similar to that indicated by the formation of hail during thunderstorms. So close did the resemblance between my soap-bubbles and those "fire balls," as they are sometimes called, appear to me that I hastened to ascertain by experiment whether or not my speculations with regard to them comprised any truth.

Soap-bubbles were accordingly blown by means of the generator containing $Zn + HCl$. Only small ones could be obtained, and these shot away towards the ceiling so quickly that it was not easy to touch them with the metallic part of the proof plane. Some few, however, were caught and the proof plane was found to be very slightly negatively electrified. Some better plan had to be tried. The end of the delivery-tube was placed under the surface of the soap solution while gas was ecaping. A great mass of bubbles were produced on the surface, and some of them toppled over the side of the dish and fell towards the floor on account of the greater quantity of water in them. These were caught on the metallic part of the proof plane, and on testing the latter it was found to be strongly charged, the sign being negative, which bore out the idea of the bubble being a charged condenser.

Another form of the experiment then occurred to me. The dish containing the soap solution was attached to an insulating handle and then set on the table. The end of the delivery-tube was once more dipped into the soap mixture, and a mass of bubbles was as before produced. The delivery-tube was then withdrawn, and the dish lifted from the table by an insulating handle. It was kept thus insulated until, in the course of a few minutes, the bubbles broke up, allowing the hydrogen to escape. The dish was then tested and found to have a negative charge. The soap-bubble blown as described was thus proved to be a charged condenser.

If by any process a mass of electrified air were enclosed by vesicles of water, or any other conducting material, a free charge would be developed on the outside which would cause the whole mass to be attracted or repelled according to the electrical state of proximate objects. In a recent number of 'Nature' I find an account of a thundercloud behaving in a manner at once explained by this hypothesis. The observer describes it as appearing to detach itself from a mass of cloud, gradually descend, and immediately after draw itself up again. The descent would be due partly to the attraction of the earth and to the repulsion of similarly electrified clouds above it. On touching the earth the free charge would escape, and the clouds, which at first repelled, would now attract, it, causing its ascent.

If such an enclosure of electrified air as I suggest were to occur at a high temperature in the upper regions of the atmosphere, its potential would gradually rise as it cooled and descended towards the earth; for the cooling of the mass of air, and the increase of pressure due to its less elevated position, would cooperate to lessen its volume, and, therefore, the surface of the enclosing film, while the quantity of electricity would remain the same.

Up to this stage I had used glass vessels in these experiments, but according to the view just set forth there could be no reason why a metallic vessel should not behave in precisely the same way. A tea-canister was accordingly filled with hydrogen by displacement of water from a generator containing zinc and hydrochloric acid. It was then placed on the insulated plate, mouth upwards, and it was found that as the hydrogen escaped into the air the spot moved to the left, as had been expected; and it was found on trial that everything that had been done with the glass flask could be done with one of metal. We have thus a condenser with only one conductor, and apparently without a dielectric; and, from the ease with which it can be charged, and most of the properties of condensers demonstrated by means of it, one which may turn out useful for teaching-purposes.

A metallic flask filled with hydrogen by displacement of water from a generator containing Zn + HCl I found very instructive. It proved that hydrogen holds its charge with unexampled tenacity, for the vessel could be handled or left in communication with the earth for a considerable time without becoming neutral. It proved that spray or vapour of hydrochloric acid had nothing whatever to do with producing the electrification, for these were absorbed by the water over which the gas was collected. It also suggested a possible means of measuring the difference of potential due to the contact of hydrogen with hydrochloric acid. After a few trials, however, I gave it up, and decided to determine the quantity by Sir W. Thomson's "Water-dropper."

A metal funnel with a long stem was fixed to the insulated plate, which was as usual connected to insulated quadrants. The funnel was filled with water, and a series of drops fell from the nozzle of the stem. The drops broke away at a distance of 6 or 7 inches from the body of the funnel. An *uninsulated* dish 3 inches in diameter, containing Zn + HCl, was placed under the nozzle, so that the drops fell into it. As the hydrogen passed into the air, enveloping the nozzle and falling drops, the spot moved rapidly up the scale to right and off it. I withdrew some of the charge from the needle, and repeated the experiment. It again went off the scale. I withdrew a considerable quantity from the needle, and eventually reduced the sensibility of the instrument to such a degree that the deflexion never amounted to 300 divisions.

Fresh zinc and hydrochloric acid were now placed in the dish. The spot rapidly went up to 130 on the scale, from zero, and began to return. The "water-dropper" was then quickly disconnected from the quadrants, and the spot stood at 125. The dish was again supplied with fresh acid and zinc and placed in position, and when I could see by the fumes that the nozzle was enveloped in the escaping gas I again connected the quadrants to the "water-dropper," when the spot moved up the scale higher. This was repeated as long as the spot could be forced to go higher. At 187 from zero it only oscillated right and left to the extent of 3 or 5 divisions, but could not be forced higher by the liberation of hydrogen near the nozzle from fresh acid and zinc. I took this number as measuring the potential above earth of the gas as it escaped from the acid.

The reason for changing the acid so frequently is to be found in the fact, which I had already proved, that when hydrogen passes through a solution of $ZnCl_2$ it comes off with a negative charge; and on this account, if the measurement be not made when the gas is passing through at the first stage of the reaction, the result cannot be correct. And, indeed, no matter what precautions are taken the result must be a little too small.

Several preliminary experiments of this kind were made, and when I was satisfied that the result was definite and constant I made very carefully the three following determinations. In each case the acid was changed *nine* times :—

	Zero.	Deflexion.	Division.
(1)	362	552	190
(2)	359	544	185
(3)	358	545	187
			3)562
			187

I knew that I had reduced the sensibility of the electrometer very much, and when I proceeded to find the value of the scale-divisions in volts I found that the standard Daniell gave only a deflexion of from 3 to 6 divisions. Besides, this being very small I could not be certain of it to within a division, as the spot did not come precisely to zero. Of course, I could not attempt to base a calculation on so uncertain a quantity.

To get over this little trouble I set up five Grove cells in series, which gave a deflexion either way of 40 scale-divisions.

Therefore,

$$\frac{187 \times 5}{40} = \text{D. P. in Grove cells.}$$

$$\frac{1{\cdot}8 \times 18{\cdot}7 \times 5}{4} = \text{D. P. in volts.}$$

$$42{\cdot}08 = \text{D. P. in volts.}$$

The other numbers given were found in the same way. The potential of an electrified body depends on conditions, and the quantity I have determined may not be what I represent it. At all events, I have found approximately the potential above earth of a point in space immediately above the surface of the hydrochloric acid, through which hydrogen is passing. The distance between the surface of the acid and the drops where they broke away separately was 2 inches. If this distance were lessened another source of error would arise (or increase), for the particles of acid propelled by the hydrogen from the acid generator would strike the stem of the funnel and lessen the electrification, because, as has been proved, they are opposite in sign to the escaping gas.

But to return to my proposed straightforward experiment. I had intended to pass a current of neutral hydrogen through a column of strong hydrochloric acid, but found it impossible to neutralize the gas from a generator. However, I had now by trial become aware that a flask of hydrogen would become neutral after the lapse of some hours.

Three half Winchester quarts were filled with hydrogen by the displacement of water from a generator containing zinc and HCl, and were left during the night to lose their charges. At 8 o'clock the next morning one was uncorked and placed on the insulated plate. The hydrogen escaped, but the spot did not move. I was then pretty certain that the gas in the other two vessels was neutral.

A beaker, 7 inches high and 2 inches in diameter, was nearly filled with strong HCl, and a very slender delivery-tube, which had been attached to one of the vessels containing the hydrogen, was placed in it in such manner that any gas escaping from the vessel should pass right through a column of acid 6 inches high. An inverted metallic goblet was now attached to the insulated plate and quadrants, and placed in such position that any hydrogen issuing from the beaker containing the acid should pass into it and be retained until it got into the air by diffusion. The hydrogen in the half Winchester quart was next forced through the acid by a stream of water. The spot did not move. No electrification was produced by the hydrogen bubbling through the acid. The experiment was repeated with the remaining half Winchester quart of hydrogen, but with the same result.

I had then to consider whether, in the face of this failure to electrify the neutral gas, I could reasonably stick to my theory of contact. I concluded that I could, and urge the following as sufficient grounds for my decision :—

I had proved beyond doubt that hydrogen holds a charge with amazing tenacity, and that it only gives it up when each molecule individually, it would appear, comes into contact with a conducting body. Such contacts were also proved to be most difficult to effect, and whatever difficulty exists in discharging the gas, by bringing it into contact with other substances, must also exist, and in a magnified form, when one tries to make contact between the gas and any substance for the purpose of charging. The difficulty of making real contact is not a new one. In fact, I hold that when the stream of hydrogen was forced through the pure acid, real contact was not made.

A very different state of things obtains when a piece of zinc is thrown into a quantity of acid. Here each molecule of hydrogen escapes from the chlorine into the acid separately, and very likely takes its charge while in the nascent condition. One cannot imitate the circumstances attending this condition of things by any possible arrangement of delivery-tubes, however small.

If it is proved that hydrogen or gases in general, in their nascent condition, take a charge when they come into contact with acids or solutions of salts, the fact, I should think, cannot fail to be of importance in the theory of the galvanic battery; and if it is proved that to make electrical contact between a gas and either metals or liquids is extremely difficult, what is known as "the air effect" in connexion with that theory appears to be negatived.

In conclusion, I wish to acknowledge the help in making many of the experiments which I have received from Messrs. Horsnell and Leach, students at present passing through the college.

St. Mary's College, Hammersmith,
November 1889.

Prof. Rücker asked if the experiment with zinc and hydrochloric acid could be started in the second stage by having the acid partly saturated with salt.

Dr. C. V. Burton thought it probable that contact could be made between a gas and a liquid by shaking them up together in a bottle.

In reply Mr. Enright said the experiment could be started at any stage, and reversal effected as often as desired by adding either acid or a solution of salt to the generating vessel.

XLI. *The Effect of repeated Heating and Cooling on the Electrical Resistance of Iron. By* HERBERT TOMLINSON, *F.R.S.*[*]

SOME of the physical properties of iron wire, even if it has been previously well annealed, can be considerably modified by repeatedly raising the metal to the temperature of 100° C., and suffering it to cool again. The internal friction, for instance, of a torsionally oscillating iron wire can be largely and permanently reduced by this process. Again, in a paper quite recently presented to the Royal Society, the author has brought forward an instance of an iron wire, which when made to go through magnetic cycles of very minute range, alternately at the temperatures of 100° C. and 17° C., was found to be permanently reduced, both in its molecular friction and its magnetic permeability, at each heating and cooling to such an extent that ultimately the former of these two physical properties became one quarter, and the latter less than one half of their respective original amounts. The large diminution of permeability and friction was also attended by a considerable lessening of the *temporary* effects of change of temperature on these properties. The object of the present investigation was to ascertain whether the electrical resistance and temperature-coefficients of iron would also be altered by the heating and cooling process. According to Matthiessen the electrical resistance of metals can be expressed by the formula

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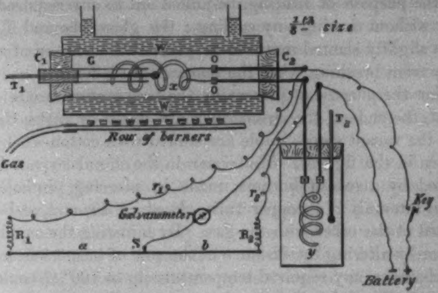
[*] Read November 15, 1889.

The Author begs to acknowledge, with thanks, the assistance which he has received, in this and kindred investigations, from the "Elizabeth Thompson Science Fund."

$$R_t = R_0 (1 + at + bt^2),$$

where R_t and R_0 are the resistances at $t°$ and 0° C. respectively, and a and b are constants. For most pure metals the coefficient a is not very far from ·00366, and the resistance varies, therefore, approximately as the absolute temperature; but with pure iron this coefficient is *much greater*. One of the questions which the inquiry was designed to answer was—Can the temperature-coefficient of iron be reduced by repeated heating and cooling to anything like ·00366? This question has been answered in the negative, but, at the same time, it appears that the electrical resistance itself suffers a small but decided change.

The iron wire examined formed part of a hank supplied by Messrs. Johnson and Nephew, and was specially prepared and annealed for the author by these makers; it was again annealed by the author himself in the following manner:—The piece of iron wire, together with several others, was placed in an iron tube about 130 centim. long; the tube and its contents were then heated, in one of Fletcher's new tube-furnaces, to 1000° C. This high temperature was preserved for several hours, and the wires were then allowed to cool slowly in the furnace. The above operations were repeated on three different days, so that eventually the wire was probably annealed as well as it could be by the ordinary method. In order to avoid any appreciable effect from the earth's magnetic force, the furnace was placed in a direction at right angles to the magnetic meridian. After the annealing the wire, which was about 120 centim. long and 1 millim. in diameter, was wrapped round with strips of paper, and wound double in a coil 5 centim. in diameter and 12 centim. in length. It was then placed in an air-chamber consisting of two co-axial copper cylinders (see fig.) connected at their extremities, and enclosing between them an annular space filled with water. The whole arrangement is sufficiently shown in the figure.



In this figure O O are two clamps grooved so as to receive the ends of the iron wire x*, the clamps themselves being soldered to stout copper connecting-rods; a German-silver wire, y, is similarly connected, and placed in a glass vessel. $a\,b$ is a platinum-iridium wire of $\frac{1}{10}$ ohm resistance; R_2 is a resistance of 100 ohms, and R_1 a set of resistances from $\frac{1}{10}$ ohm to 200 ohms; the india-rubber-covered connecting-wires, r_1, r_2, have each a resistance of $\frac{1}{20}$ ohm at the temperature of 17° C.; and S is a sliding-piece which, by a suitable spring-and-catch arrangement, can be kept pressed on any part of ab. At the temperature of the room, the resistance of y is nearly equal to that of x, and R_1 as well as R_2 is nearly 100 ohms; consequently, any slight changes which may occur during the experimenting in the temperature of r_1, r_2, and $a\,b$ do not sensibly affect the balance. The thermometers T_1 and T_2 register the temperatures of x and y respectively †; T_1 has its bulb in the centre of x, and is enclosed by the glass tube G; this tube is lined with india-rubber at the extremity outside the air-chamber, so that the thermometer can, if necessary for the purpose of reading, be pulled out to any required extent without causing any cooling; the glass tube and T_1 are both slightly slanted upwards to prevent the column of mercury from breaking when the temperature is falling.

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* If the grooves, and the ends of the wire which fit into them, be well cleaned, clamping serves quite as well, for the purpose of connexion, as soldering, and is more convenient.

† Only one thermometer was used for T_1 in this particular investigation, but the author generally employs, in work of this kind, three thermometers in turn. One of these thermometers is graduated from −5° C. to 30° C., the second from 30° C. to 65° C., and the third from 65° C. to 100° C.; all three thermometers are divided into tenth degrees, and have been carefully compared with the Kew standards.

For the purpose of preserving the temperature inside constant, the ends of the air-chamber are closed by corks, C_1, C_2, and the vacant spaces inside are stuffed with cotton-wool (not shown in the figure). The water in the annular space W is heated by a row of burners, made by piercing pin-holes at equal intervals in a copper tube, closed at one end, and connected at the other with the gas. By adjusting the supply of gas or by altering the distance of the row of burners from the air-chamber, any required temperature up to 100° C. could be maintained nearly, if not quite, constant for a considerable length of time. In 15 minutes the water could be raised to 100° C.; and then, by diminishing the supply of gas so that the water only just boiled, this temperature could be kept up for 16 hours without adding more water; when it was necessary to maintain the temperature at 100° C. for longer periods of time, the vessel was replenished with boiling water.

It will be seen from the figure that there is a "Wheatstone's Bridge" arrangement for determining the resistance-ratio, $x : y$. The temperature of y varied by only a few degrees during the whole of the inquiry; and as both the actual resistance of the coil at 17° C. and its temperature-coefficient were known, the resistance of y at any other temperature could be calculated.

It is usual in experiments with the "bridge" to close first the battery-circuit, and immediately afterwards that of the galvanometer; but the author prefers to keep the galvanometer-circuit always closed, and to observe the effect of closing the battery-circuit. It is true that in the latter case there is always a momentary throw of the needle due to self-induction in the wires, but this, with the arrangement shown above, is very slight; and with no more delay than that of two or three seconds, it can be easily ascertained whether the act of closing the battery-circuit causes *any alteration in the difference* *of potential at the galvanometer-terminals*[*]. If, on the contrary, the former of the two methods be employed, there arises more or less inaccuracy from the presence of thermo-electrical currents set up in one or more branches of the bridge by slight variations in the temperature of the air at different parts of the room. Besides, even if the air could be maintained at perfectly uniform temperature throughout, thermo-electrical currents are always produced by the act of pressing down the sliding-piece, S, on the wire *a b*, and by Peltier effects.

The act of bending the wire into the form of the coil produced, as might be expected, a slight permanent change in its resistance, which was thereby increased by ¼ per cent.; doubtless a portion, but only a small one, of the whole reduction of resistance, to be presently recorded, is to be attributed to the partial or complete removal of the effect of coiling.

When everything was ready, and the wire had been left undisturbed for several hours, its resistance and temperature were determined; the latter was then raised to 100° C., or very nearly to 100° C., and maintained thereat for at least 8 hours[†]; and during this period the resistance of the wire was tested several times. The wire was then permitted to cool down, and its resistance was again determined about 16 hours afterwards. The same operations were repeated again and again, until the metal showed that no sensible change of resistance was produced by the heating and cooling. The results are given in the following Table (p. 322).

It will be noticed that the specific resistance both at 17° C. and at 100° C. is diminished with each repetition of the heating and cooling process, until, finally, it becomes about 4½ per cent. less than at first. Thus, though the permanent effect on the electrical resistance is very much less than that on the permeability for very minute magnetizing forces, it is quite sensible.

At the sixth heating the wire was kept at 100° C. for 26 hours and, moreover, the battery-current was applied first in

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[*] There is always a difference of potential at the galvanometer-terminals arising from thermo-electrical effects; see what follows.

[†] The deviation from 100° C. never exceeded $\frac{3}{10}$° C., and could in all cases be accounted for by changes in the barometric pressure.

Number of times heated to 100° C.	Specific resistance in C.G.S. units at 17° C. R_{17}.	Differences between consecutive values of R_{17}.	Specific resistance at 100° C. R_{100}.	$R_{100}-R_{17}$*.	$\frac{R_{100}-R_{17}}{R_{17}}$*.	Remarks.
0	11162					
1	10942	220	16029	4952	·4527	Kept at 100° C. for 8 hours; the battery-current always applied in the same direction.
2	10807	135	15796	4922	·4526	Ditto.
3	10772	35	15789	4985	·4613	Ditto.
4	10757	15	15747	4999	·4633	Ditto.
5	10755	2	......	4996	·4638	Ditto.
6	10727	28	15724	......	......	Kept at 100° C. for 26 hours; the battery-current applied first in one direction and then in the opposite.
7	10711	16	15679	4983	·4629	Kept at 100° C. for 8 hours; the battery-current applied first in one direction and then in the opposite.
8	10694	17	15674	4965	·4636	Ditto.
9	10689	5	......	......	......	Ditto.
10	......	......	15654	......	......	Ditto.
11	......	......	......	......	......	Ditto.
12	10688	......	......	......	......	Ditto.

* For the mode of calculating the numbers in these columns, see what follows.

one direction and then in the opposite. These reversals of the current served to increase the rate of diminution of the resistance.

The maintaining the temperature at 100° C. for 26 hours did not diminish the resistance as much as the next two heatings and coolings taken together; evidently, therefore, the cooling exerts an influence as well as the heating. It is not, in fact, of much use prolonging each period of heating beyond 8 hours, whilst, on the contrary, a less time will hardly suffice, heating and cooling in rapid alternation being comparatively ineffectual.

Though the specific resistance of the annealed iron is permanently diminished by the heating and cooling process, this is not so with the *temporary* change of resistance arising from change of temperature; the sixth column of the Table shows that the temporary increase of resistance produced by raising the temperature from 17° C. to 100° C. is not affected by the heating and cooling process, whilst the temporary increase of resistance *per unit* becomes greater in proportion as the specific resistance itself becomes less.

The values of $R_{100}-R_{17}$ and $\frac{R_{100}-R_{17}}{R_{17}}$ were calculated so as to avoid, as much as possible, error arising from the *permanent* changes consequent on the heating and cooling. Let C_1, C_2, C_3, &c., represent the resistances of the cold wire after the first, second, third, &c., heatings, and H_1, H_2, H_3, &c., the corresponding resistances of the hot wire, then the numbers in the sixth column were obtained from $\frac{H_1+H_2}{2}-C_1$, $H_2-\frac{C_1+C_2}{2}$, $\frac{H_2+H_3}{2}-C_3$, $H_3-\frac{C_3+C_4}{2}$, &c., and those in the last column by dividing these by C_1, $\frac{C_1+C_2}{2}$, C_3, $\frac{C_3+C_4}{2}$, &c., respectively. Of course, the temperature of the room was not always 17° C., but the resistance of x at 17° C. could be calculated from its actual resistance at the temperature at the time by means of a formula ultimately obtained for the resistance of the wire at any temperature*. This formula was calculated from the results of a number of very careful observations at 17° C., 60° C., and 100° C., and after the heating and cooling process had ceased to affect the resistance. The formula thus obtained was :—

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* Similarly for the higher temperature of 100° C.

$$R_t = R_0 (1 + \cdot 005131\, t + \cdot 00000815\, t^2);$$

whilst the formula deducible from Matthiessen's results for *pure* iron annealed in hydrogen is

$$R_t = R_0 (1 + \cdot 005425\, t + \cdot 0000083\, t^2).$$

The specific resistance at 0° C. ($R_{0 \cdot T}$) of the author's iron was 9808 electromagnetic units, whilst that of Matthiessen's pure iron ($R_{0 \cdot M}$) is given on the authority of the late Prof. Jenkin as 9718[*]. It seems not improbable that this last value is from 4 to 5 per cent. too high ; for it follows from Matthiessen's researches that the resistances at any temperature of a pure metal and its alloy should be in the inverse ratio of the rates of increase of resistance at that temperature, so that

$$\frac{R_{0 \cdot M}}{R_{0 \cdot T}} \text{ should equal } \frac{\cdot 005131}{\cdot 005425},$$

which, if the author's result be assumed to be correct, would make $R_{0 \cdot M}$ equal to 9277.

Unfortunately, Matthiessen did not determine the *absolute* resistance of iron and many of the other pure metals examined by him, and it appears from Prof. Jenkin's own statement[†] that the calculated results for these metals cannot be depended on for any great degree of accuracy. The author, therefore, ventures to express the hope that the B. A. Electrical Standards Committee may be induced to determine the absolute resistances and the temperature-coefficients of those of the pure metals which are in ordinary use.

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[*] The number actually given by Prof. Jenkin has been multiplied by ·9889 (the value of the B.A. unit in terms of the legal ohm).

[†] See note on p. 250 of Prof. Jenkin's book on 'Electricity and Magnetism.'

Dr. Walmsley asked for what value of the magnetizing force the permeability of the iron mentioned in the beginning of the paper was determined; to which Mr. Tomlinson replied that they were much smaller than the earth's horizontal component.

XLII. *The Electrification of a Steam-jet.*

By Shelford Bidwell, *M.A., F.R.S.*[*]

The boiler used in these experiments consists of a small tin bottle capable of holding about fifteen fluid ounces, which is supported upon a tripod above a Bunsen burner. Its neck is fitted with a cork, through which passes a glass tube $\frac{5}{16}$ in. in diameter, terminating in a nozzle with an opening of about $\frac{1}{16}$ in. At a point four inches above the cork, the tube is bent to an angle of 100 or 110 degrees, and the nozzle may be formed at a distance of five or six inches from the bend. In this case its direction will, of course, be nearly horizontal. When it is desired to have a vertical nozzle, the tube is again bent in an upright direction at a distance of not less than five inches from the first angle, the nozzle terminating two or three inches above the second bend. The nozzle is made by simply drawing out the tube to a point in a flame, and cutting off its end at the proper place.

I have given these dimensions in detail, not, of course, because they are absolutely the best possible, but in order that anyone who wishes to repeat the experiment may be enabled to obtain good results without trouble. The object aimed at is to keep the tube as free as possible from water, whether arising from ebullition or from condensation, so that the jet of steam may be regular and uniform; at the same time, it is essential that the end of the nozzle from which the steam issues should not be directly above the gas-burner, or the experiment cannot succeed.

The tin bottle is charged with four ounces of water, and when it boils the burner is adjusted until the steam-jet is as vigorous as is possible consistently with steadiness; if the ebullition is too violent, the water thrown up into the tube will produce jerks, masking the effect to be looked for.

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[*] Read December 6, 1889.

The jet is most conveniently observed by means of its shadow projected upon a white screen. For this purpose a lime-light is desirable, but any good source of light may be employed. The shadow of the jet will be seen to be of feeble intensity and of a neutral tint; the jet is, in fact, nearly transparent, and does not appear to exercise any selective absorption upon the light. If now a sharp point, or, better, a small bundle of points, in connexion with one of the terminals of an influence-machine (such as a Voss), is brought near the base of the jet, the machine being worked slowly, a startling change instantly comes over the shadow. It becomes dark and dense, at the same time assuming a marked orange-brown tint. By removing the point, or simply connecting it to earth, the shadow can be immediately caused to resume its original appearance, again becoming almost invisible. This operation may be repeated as often as desired, one of the most remarkable things about it being the extreme rapidity with which the changes are produced.

The point need not necessarily be directed to the origin of the jet; it is nearly as effective when placed in the steam at a distance of a foot or more from the nozzle; and in such a case the *whole* of the steam-jet is equally acted upon from its origin onwards, even when the direction of the point is the same as that of the jet. A ball may be used instead of a point, but it operates only when actually within the jet, whereas a point may be outside it. The effect may also be produced by directing the jet upon an electrified metal disk, supported at a considerable distance from the nozzle.

I have examined the absorption-spectrum of the steam-jet. When the jet is not electrified, its action upon the spectrum is small, the intensity of the whole being slightly diminished in an apparently equal degree throughout. Possibly the violet is dimmed in a somewhat greater proportion than the other colours. Electrification of the jet causes the violet to disappear completely, while the luminosity of the blue and the more refrangible part of the green is materially decreased. The orange and red are, I believe, quite unaffected.

From these facts it may be concluded that electrification causes an increase in the size of the water particles contained in the steam-jet. In the unelectrified condition the majority of these particles are small in relation to a wave-length of light; under the influence of electrification they become larger, and attain a diameter of something like a fifty-thousandth part of an inch.

The idea naturally suggests itself that the phenomenon may be of the same nature as that observed by Lord Rayleigh in the case of water-jets*. A stream of water issuing in a nearly vertical jet from a small orifice is found to break up into separate drops at a certain distance above the orifice. Under ordinary conditions these drops collide with one another, and, again rebounding, become scattered over a considerable space. But when subjected to the influence of an electrified body, as a rubbed stick of sealing-wax, brought near the point of resolution, the colliding drops no longer rebound but coalesce, and the entire stream of water, both ascending and descending, appears to become coherent. Now it seems to me certain that the innumerable minute particles of water generated in the steam-jet, each consisting perhaps of only a few molecules, must necessarily come into frequent collision with one another; for we cannot suppose that they all travel with equal velocities and in exactly the same direction; and there is no reason why they should not behave just in the same manner as the larger drops of the water-jet, rebounding when they are not electrified, coalescing into larger drops when they are. It is true that the degree of electrification required to produce the phenomenon in the case of the steam-jet is apparently much greater than in the other, a rubbed stick of sealing-wax being altogether inoperative. Perhaps this is a consequence of the different sizes of the drops in the two cases. That the actual electrification of the particles in the steam-jet is really very small indeed, is proved by the fact that if two electrified steam clouds are generated in close proximity to each other, they exhibit little, if any, evidence of mutual repulsion when they are similarly electrified, or of attraction when their electrifications are of the opposite kind.

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* Proc. Roy. Soc. 1879.

Lord Rayleigh shows cause for believing that if the electrifications of the drops in the water-jet were strictly equal, the phenomenon in question would not occur; and the reasons why they are not, in fact, equal would just as well apply to the case of the steam-jet. How unequal charges of electricity of the same name are operative in bringing about coalescence Lord Rayleigh does not explain, nor am I prepared to hazard a conjecture on the point.

Lord Rayleigh concludes his paper by referring to the importance of the investigation from a meteorological point of view. I may do the same. It seems certain that the steam-jet experiments go far towards explaining the cause of the intense darkness which is characteristic of thunder-clouds, as well as of the lurid yellow light by which that darkness is frequently tempered.

I had made the above described experiments and drawn the above stated conclusions concerning them, and had just requested the Secretary of the Physical Society to accept the present communication, when Prof. Silvanus Thompson, whose knowledge of scientific history is proverbially encyclopædic, was good enough to bring to my notice the fact that experiments upon the electrification of a steam-jet had been recently made in Germany by the late Robert Helmholtz. An account of these experiments, which are of the greatest interest, is given in Wiedemann's *Annalen*, vol. xxxii. p. 1. The author appears never to have examined his jets by transmitted light, and makes no mention of the marvellous increase of opacity under electrification, to which I have called attention. His observations were all made against a *dark* background, the illuminating beam of light coming obliquely from the front, but shaded from the eyes. When the jet is electrified, he says, it is at once seen more clearly and sharply against the background; it also assumes diffraction colours, like those seen in strata of fog. Very strong electrification produces a deep blue colour, indicating the formation of very small mist drops. On slowly diminishing the electrification the blue tint at first becomes gradually paler, which points to the formation of larger drops; and it is then succeeded by tints of purple, red, yellow, green, and finally, when the discharge is very feeble, by pale blue tones again. Under certain circumstances all these several tints may be seen at once in different portions of the jet.

I, too, had observed these diffraction colours, which were sometimes very beautiful; but having convinced myself that they undoubtedly occurred at times when no electrical influence (that I knew of) was operating, I did not follow up the observation.

Helmholtz found that these condensation phenomena could be just as well produced by some other causes as by electrification, and especially by the agency of flame. All the exciting causes in question involve some continuous chemical action inside the steam-jet. He conjectures, therefore, that the sudden condensation may be due to molecular tremors or shocks, which upset the unstable equilibrium of the supersaturated vapour, just as a small disturbance will sometimes cause the sudden crystallization of a supersaturated saline solution; and after showing that chemical reactions may occur freely even outside the visible portion of a flame, he states his opinion that similar dissociations and recombinations take place among the molecules of the air affected by an electric discharge, as evidenced, for instance, by the formation of ozone.

A very striking illustration of the effect of combustion is afforded by holding beneath the jet a piece of burning touch-paper, made by soaking blotting-paper in a solution of nitrate of lead. The jet at once becomes quite as opaque as if electrical influence were employed, and the same hissing noise is also heard. The burning touch-paper suggested itself as more convenient than the actual flames used by Helmholtz for the purpose; for these cannot be brought underneath the jet without causing its immediate dissipation; and it is therefore necessary to hold them on one side, and direct their heated gases upon the jet by blowing or fanning.

After reading Helmholtz's paper it naturally occurred to me to try the effect of combustion upon the water-jet. In this case the objection to the use of an actual flame does not, of course, apply. I therefore introduced the flame of a Bunsen burner into the jet, and found that, if the air-holes were stopped up, and the flame thus rendered luminous, a decided effect was produced upon the jet. Often, indeed, it became just as completely coherent throughout its length as if it were electrified, though I have not yet succeeded in obtaining this result with perfect certainty; but in every case when the burner was held a little below the point of resolution, a considerable diminution of the scattering was observed. If the air-holes were opened and the flame made non-luminous, it failed to act in any way whatever upon the jet. It is to be remarked that Helmholtz found certain non-luminous flames (of which, however, a gas-flame was not one) to be devoid of influence upon the steam-jet.

Can the effect here also be due to electrification? It can hardly be attributable to heating, because the flame of a Bunsen burner is certainly hotter when it is non-luminous than when it is luminous. On the other hand, it is known from many experiments that flame is electrified. If, for example, we place a luminous flame between the positive and negative ball-terminals of a Voss machine in action (the terminals being too far apart for sparks to pass), we find that the flame, or at least the upper part of it, is repelled by the positive and attracted by the negative ball. This seems to denote positive electrification of the flame.

In conclusion, it should be mentioned that Helmholtz suggests another hypothesis as an alternative to that of molecular shock, though he does not appear to attach much importance to it. It is that the electrical discharge, the flame or other exciting agent, whatever it may be, acts by introducing into the steam-jet minute particles of solid matter which serve as nuclei upon which the water-vapour may condense, as in Aitken's experiments on the formation of fog.

It is possible that several different causes may be competent to produce the condensation phenomena.

Prof. Rücker said Mr. Bidwell had no doubt considered the question, but he should like to ask him whether he thought the behaviour of the jet when the burning paper was held under it might not be due to a lowering of its surface-tension. Though the length at which a liquid cylinder becomes unstable is independent of the tension, the rate at which the transformation into drops takes place depends on it, and thus the distance from the orifice at which discontinuity is established would vary with the state of the surface.

Mr. Richardson inquired whether a red-hot iron had any effect.

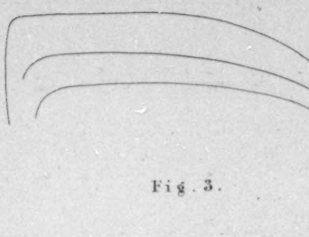

Fig. 3.

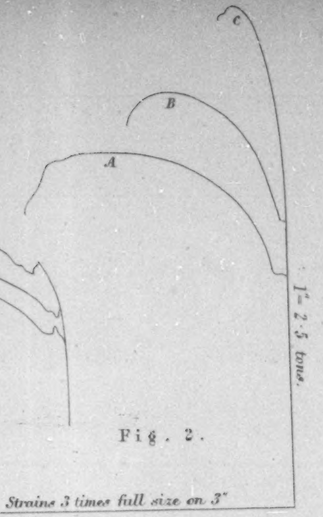

Fig. 2.

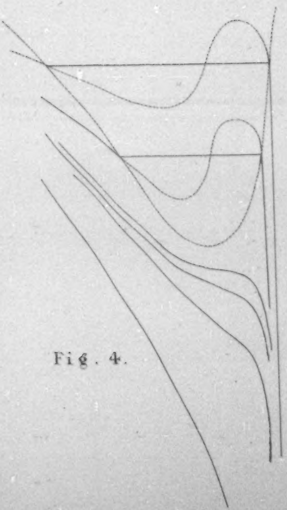

Fig. 4.

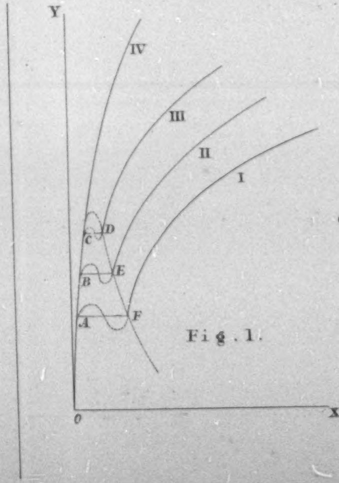

Fig. 1.

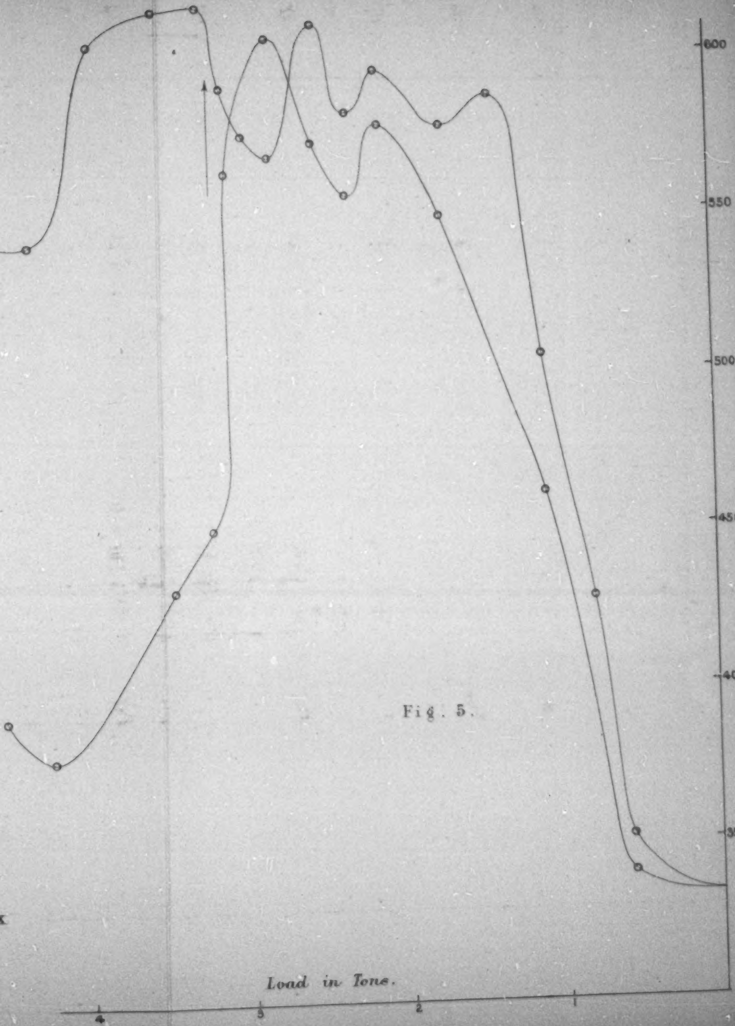

Fig. 5.

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Dr. Fison said he had made experiments on the electrification of flame, and found that potentials varying from +2 volts to −1½ volt could be obtained in the region within and surrounding a Bunsen-flame.

Prof. S. P. Thompson commented on the contrast between Mr. Bidwell's experiments and those of Dr. Lodge on the dissipation of fogs by electricity, and also asked whether the colour of the jet depended on the length of spark produced by the machine.

Prof. Forbes thought a crucial test between the two hypotheses of Helmholtz could be obtained by trying the experiment in a germless globe.

The President said he had recently noticed that gas-flames were electrified.

Mr. Bidwell, in reply, said that since writing the paper it had occurred to him that the effect of flame upon the water-jet might possibly be due rather to the introduction of dirt into the stream than to electrification, Lord Rayleigh having observed that scattering did not occur if a little soap or milk were added to the water. He thought that the colour assumed by the electrified steam-jet appeared generally to be independent of the sparking-distance. He had not tried whether a red-hot iron produced any effect.

XLIII. *On the Behaviour of Steel under Mechanical Stress.* By C. A. Carus-Wilson, *B.A., A.M.I.C.E., Demonstrator in the Mechanical Laboratory at the Royal Indian Engineering College, Coopers Hill*[*].

[Plates IX.-XI.]

The effect of uniform longitudinal stress on a steel bar is threefold:—

(i.) The molecules are strained.

(ii.) The elements are strained.

(iii.) In virtue of the straining of the elements "flow" is produced.

The strain usually observed is the elongation due to flow, which may be recoverable or irrecoverable. The strains of an element can be shown to be a uniform dilatation combined

[*] Read December 6, 1889.

with a uniform shear about an axis parallel to that of the bar; hence the elongation due to flow consists of a sliding combined with an increase of volume.

The state of strain of an element may be conveniently described by defining the shape, size, and orientation of the strain quadric, which is a sphere of radius $3\sqrt{\frac{k}{p}}$ combined with an hyperboloid of two sheets whose principal semi-diameter is $\sqrt{\frac{3n}{p}}$ and axis parallel to that of the bar; where p is the intensity of longitudinal stress, n the rigidity, and k the bulk modulus.

Up to a certain stress the elastic properties of the steel are such that the strain produced is recovered when the load is removed, but at that stress the elastic resistance is overcome, and the molecules slide over one another, the strain being permanent.

This sliding beyond the limit of elastic resistance does not continue. The elastic resistance is raised by the sliding, and consequently the sliding is limited.

A stress-strain curve can be drawn which shows the increase of the limit of elastic resistance ρ with sliding; the rate of increase of the former at any point may be measured by observing the angle which the tangent to the curve at that point makes with the axis of strain; I will call this angle ϕ.

The initial value of ρ is often taken as an indication of the "hardness" of the steel, and when ρ is raised by permanent strain the steel is said to be "hardened" thereby. This, however, conflicts with a measure of hardness usually employed, namely the amount of permanent strain caused by performing a given amount of work on a bar.

Thus if a hard steel edge attached to a weight is allowed to fall on the surface of a steel bar, the depth of the indentation is taken as a measure of the softness of the metal.

If we draw the stress-strain curve O P for the steel under examination, the work done in producing a permanent strain O D is the area O P D. If O Q represent the stress-strain curve of a harder bar, the same amount of work, O Q C, will clearly produce less permanent strain, O C. The hardness then is evidently determined by the form of the curve; but

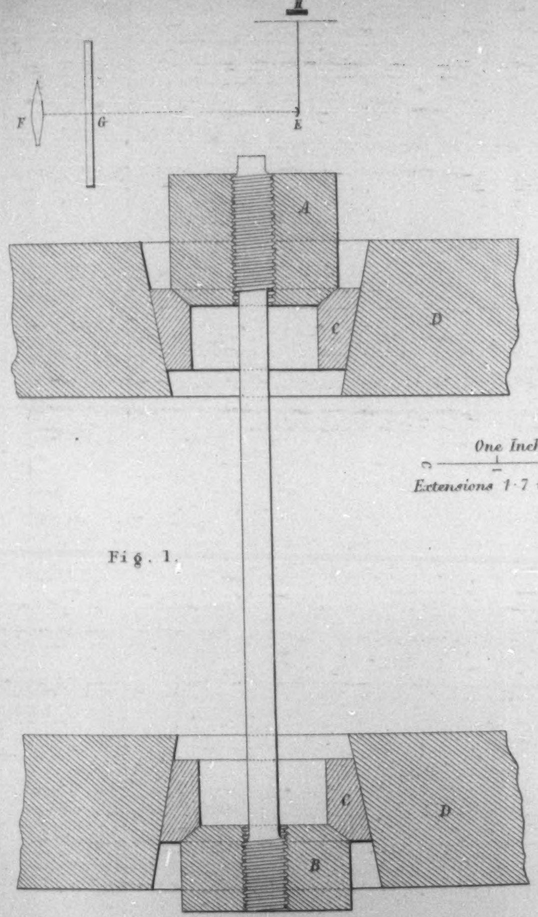
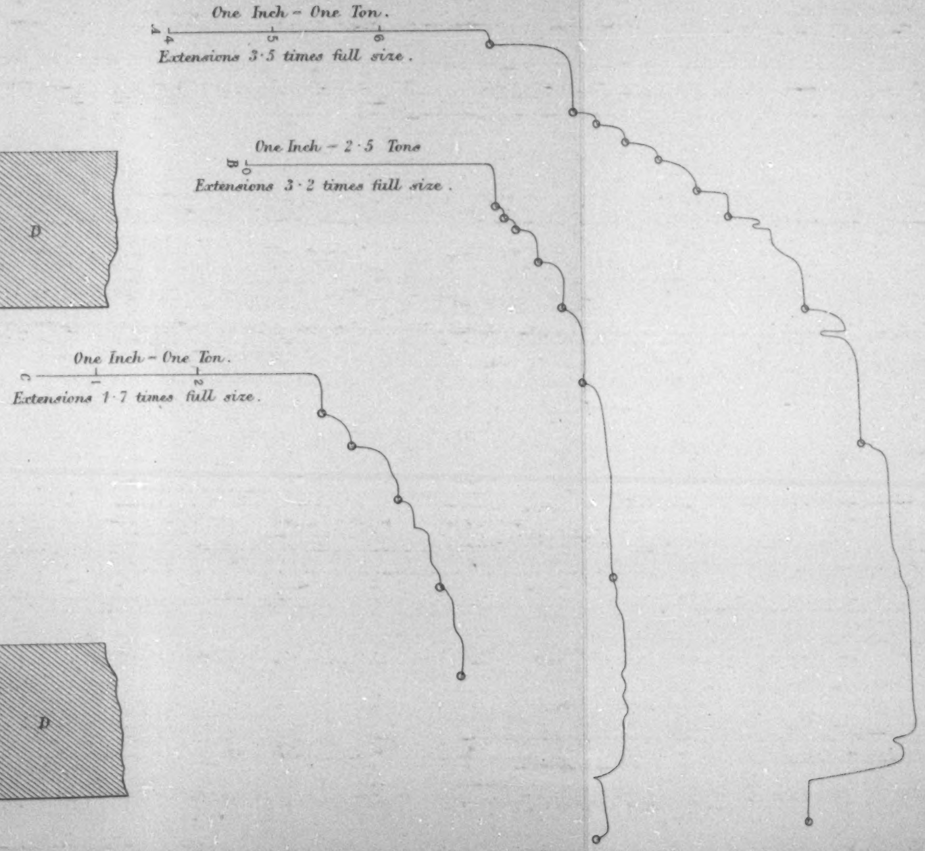


Fig. 1.



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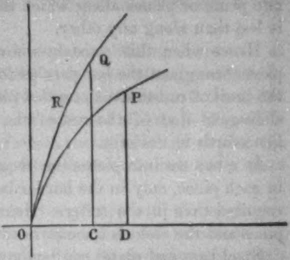

this depends upon the rate of increase of ρ with permanent strain, *i. e.* upon the value of ϕ. Hence $\tan\phi$ is the true measure of the hardness; but $\tan\phi$ is not constant, it diminishes: therefore the initial value of $\tan\phi$ must be taken as the measure of hardness. It will be seen that ρ cannot be taken as a measure of the hardness, for ρ is the same at R and P for the two bars, but the former is obviously the harder*.

We can say, then, that permanent strain raises the limit of elastic resistance, but diminishes the hardness—defined as the rate of increase of ρ with permanent strain.

I imagine it is because the harder steel has the greater value of initial ρ that ρ has been taken as a measure of the hardness.

If a bar of soft steel be taken and cut in four pieces; No. II. "hardened" in oil; No. III. in water [alkaline]; No. IV. in water [acid]; if each piece is now tested we shall obtain stress-strain curves as in fig. 1, Plate IX. A curve can be drawn through the points O A B C D E F, which I will call the "yield-line"; this will be the locus of all points such as A and F for pieces of the same bar "hardened" to different degrees. The points A, B, C are called the "yield-points."

As the piece is harder the yield-point—or initial limit of elastic resistance—is higher, and the yield A F is less†.

When the yield begins it continues until ρ is raised by a given amount λ. If we could show that λ was the same for each piece, then it would follow that the yields as A F, B E, C D are direct measures of the hardness of the piece.

If there is any inequality or want of homogeneity in the bar due to unequal stresses in the manufacture, there may be one plane or planes along which the limit of elastic resistance is less than along any other.

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* If $\tan\phi=0$, the bar at that point is perfectly plastic.

† Fig. 2, Plate IX. is a facsimile of an autographic diagram thus obtained: the steel bar was cut in three pieces; A was soft, B hardened in oil, and C in acid.

Hence when this resistance is overcome sliding will take place throughout the bar parallel to that plane or planes until the limit of resistance along that plane has been raised, by the sliding, to that of the rest of the bar, when the sliding will henceforth be uniform.

In a bar cut into pieces the required increase is the same in each piece, only in the harder bar less permanent strain is required than in the softer. Hence λ is the same in each piece and the yield is a measure of the hardness.

Steel bars and plates can be shown in which the permanent strain at the yield-point has clearly taken place as a sliding parallel to one plane only.

It would seem that there was an apparent discontinuity in the form of the stress-strain curve at the yield-point.

The question arises whether the curve may not actually be continued through a double inflexion, and be one with the rest of the curve as shown by the dotted lines, fig. 1, Plate IX.

Let us imagine a solid model made so as to represent at any point of its surface a particular condition of the bar with respect to stress, strain, and hardness. Let the curves I., II., III., IV. (fig. 1, Plate IX.) be placed parallel to one another at distances from the plane X O Y proportional to the hardness which each curve represents, with their origins in the axis O Z (perpendicular to X O Y), and let the surface of the model be made to pass evenly through all these curves and other similar curves which would be obtained at higher and lower degrees of hardness.

We see that from the curve which just passes outside the yield-line there is a part of the solid figure entirely wanting. It would seem a much more natural view to suppose that in some sense the successive curves are theoretically continuous, as shown by the dotted line above.

Such a supposition involves a state of affairs in which the limit of elastic resistance decreases as the permanent strain increases.

This would be the case if the sliding at the yield-point instead of taking place simultaneously over the whole bar parallel to one plane only took place as a strain wave passing up or down the bar with a definite velocity without any further increase of load.

This is what actually takes place. Sliding is started at one end and takes place along a definite plane, generally at 45° to the axis; this disturbs the equilibrium of the molecules in the parallel plane next to it, and sliding is induced in this plane, so that a wave of strain passes the whole way up the bar; or sometimes starting from both ends two waves meet in the centre.

The velocity of the wave varies greatly with the quality of the steel; sometimes the wave appears to be instantaneous, sometimes so slow that it may take 30 seconds to pass up a 10-inch bar, in which case the pencil drawing the stress-strain curve is seen to follow the wave; in the former case the pencil drops as if the bar had broken.

When the wave starts from one end a plane in the centre experiences no permanent sliding until the wave has reached the plane next to it, so that while originally the resistance to sliding was greater than the stress, now it is less; in other words, permanent strain (in an adjacent plane) diminishes the resistance to permanent strain.

The most reliable stress-strain curves are those drawn by Prof. Kennedy's apparatus, and these show a rapid and irregular inflexion at the yield-point (see fig. 3, Plate IX.). In these diagrams the record of stress is unaffected by the inertia due to the load.

These stress-strain curves indicate the condition of strain in a steel bar as, by gradually increased stress, the steel is converted from an elastic solid to a viscous fluid. It is interesting to compare such curves for steel of different hardness with the stress-strain curves of a gas at different temperatures (see fig. 4, Plate IX.).

We have the effect of increased $\left\{\begin{matrix}\text{hardness}\\ \text{temperature}\end{matrix}\right\}$ doing away with the discontinuity at the $\left\{\begin{matrix}\text{yield}\\ \text{boiling}\end{matrix}\right\}$ line. Also the strong probability that in both cases the apparent discontinuity is really a double inflexion due to a change taking place piecemeal throughout the substance, and inconceivable if the substance be supposed homogeneous (*cf.* Prof. J. Thomson, Proc. Roy. Soc. 1871, no. 130).

[The stress-strain curves shown in the diagram are from Dr. Andrews's paper (Phil. Trans. vol. clix. 1869, p. 575).]

Thus far the strains considered have been those due to relative displacements of the molecules, but it is impossible to conceive that this can take place without the molecules themselves being strained, *i. e.* without a displacement of the atoms.

Such a displacement of atoms, if permanent, would alter the chemical and physical condition of the iron. Hence we must look to the straining of the atoms to account for the change that takes place when a bar is permanently strained, *i. e.* to account for the increase in the value of ρ with permanent strain.

Osmond has proved almost beyond a doubt that the so-called "hardness" of steel is due essentially to the existence of an allotropic modification of iron; that the molecules of iron may be either (α) soft, or (β) hard. (*Cf.* Osmond's *Etudes Métallurgiques*, Paris, Dunod, 1888.)

Hence the change induced in steel by permanent strain must be due to a permanent straining of the atoms in the molecule.

We should then expect to find evidence of a straining of an entirely different order and nature to that due to a molecular disturbance. Such evidence is forthcoming.

It has long been known that if a steel bar lying in a magnetic field of limited intensity be subjected to tension in the direction of the magnetizing force, the magnetization of the bar will be increased.

I have made a series of experiments on this subject, and find that the intensity of magnetization increases regularly with the load up to the yield-point.

The following experiments were made:—

The bar to be tested was turned in the lathe, and a screw cut on each end; it was placed vertically in the testing-machine; to each end was screwed a nut, the top one (A) of gun-metal, the lower one (B) of steel (see fig. 1, Plate X.). The former was of gun-metal so as to remove the free upper extremity of the bar as far as possible from the neighbourhood of large masses of magnetic material. These nuts rest on steel collars, C C, which have conical surfaces to fit into conical holes in the main shackles, D D, of the testing-machine. The bearing of the nuts on the collars is spherical, so as to

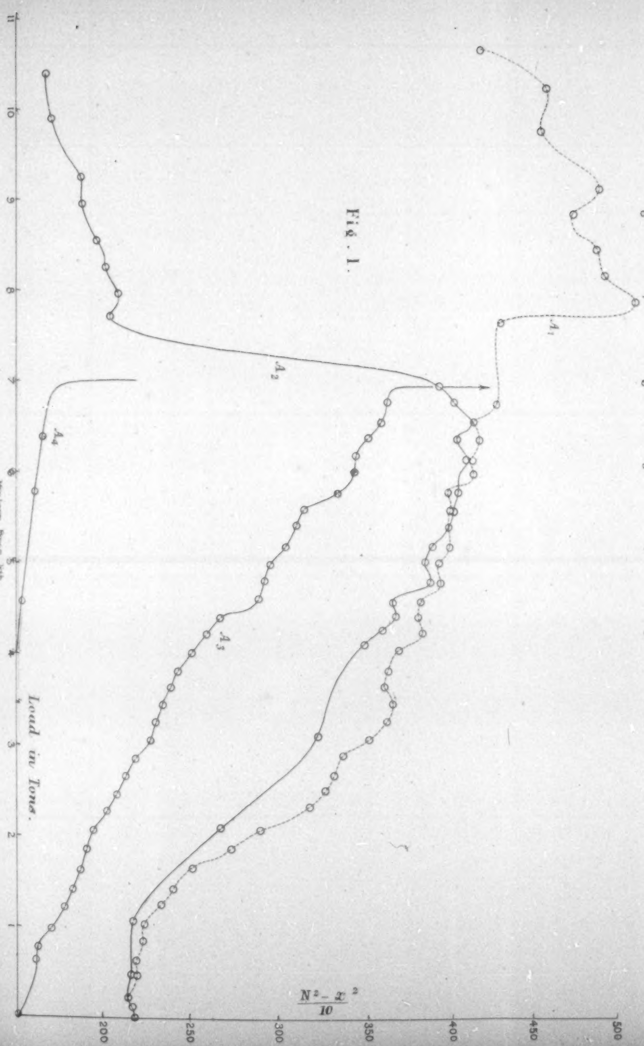

Fig. 1.

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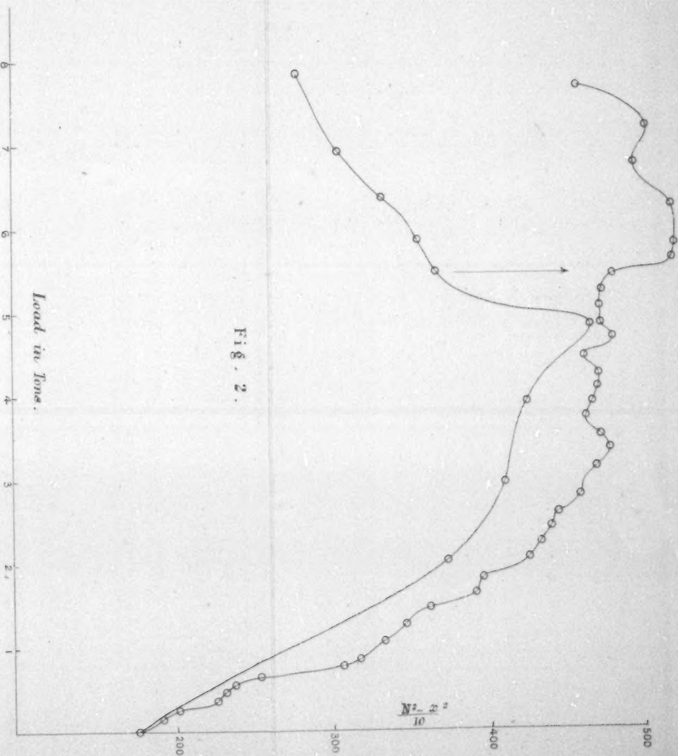

Fig. 2.

allow of a free movement of the specimen, and to ensure a direct pull. A short distance above the upper end of the bar a small magnet, E, is hung by a silk fibre, the suspension of which is rigidly attached to the upper shackle, so that any small motion of the latter involves the same motion of the needle; a silvered mirror is attached to the needle, which reflects a beam of light from a lamp, F, onto a horizontal scale, G; H is a controlling-magnet.

A small displacement is given to the magnet (i.) when there is no load on the bar, and the time of 20 complete oscillations observed; (ii.) the same with a small load on; (iii.) the load is then removed, and the time of 20 oscillations again observed. The results are recorded in the accompanying Table.

Experiment I.—Bar of soft steel, never loaded before: cross sectional area 0·306 square inch, 19·5 inches long; elongations measured on 10″ by a vernier and microscope reading to thousandths of an inch.

An automatic stress-strain diagram was taken (see curve A, Plate X.). The time of 20 oscillations of the needle was first observed with the bar out—102·6 seconds. From this was deduced x^2, the square of the number of oscillations per minute, and this was deducted from N^2, the same when the bar was in place. The results are plotted on fig. 1, Plate XI., as two curves, the upper one, A_1, being the curve of temporarily-induced magnetism, with load on, and the lower one, A_2, that of permanent magnetism, with load off. The arrow shows the yield-point. The two curves cross one another twice, and coincide in a third point. The third curve, A_3, is the curve of extensions from the second column of the Table, 186 times full size. Curve A_4 is the curve of permanent set, same scale as A_3. This bar was broken. The last observation was after the fracture.

Experiment II.—Bar of soft steel never loaded before: cross-sectional area 0·308 square inch, 19 inches long; diagram measures elongations of 10″; square of number of oscillations per minute with bar out 121; ditto, bar in, no load on, 1849. The results are plotted as a curve in fig. 2, Plate XI. The arrow shows the yield-point. The observations at this point were taken after the set. The stress-strain diagram is shown at B, Plate X. This bar was broken.

Experiment III.—Wrought-iron bar never loaded before: cross-sectional area 0·203 square inch. The extensions on the diagram, C, Plate X., are on a length of six inches. The results are recorded in the Table, and plotted on fig. 5, Plate IX., the ordinates here representing simply the square of the number of oscillations per minute. The arrow shows the yield-point. The curves intersect twice. This bar was not broken.

The circles show the loads at which observations were made.

From the curves on Plates IX. and XI., it is evident that the magnetic induction is increased in a regular way by increased stress, and that the effect is largely permanent; a point is reached, before the yield-point, where the effect produced is wholly permanent. The two curves continue side by side up to the yield-point, when the large permanent set which then takes place completely upsets the former condition of the bar, as might have been expected. The temporary effect is roughly proportional to the elastic extension below the yield-point.

The conclusions to which I am drawn by these experiments are:—

(i.) Mechanical straining produces an "atomic disturbance" in a bar which increases regularly with the stress.

(ii.) For small stresses the disturbance is only partly permanent, but as the yield-point is neared it becomes wholly permanent.

(iii.) The magnetic properties of a loaded bar are in general different from those of the same bar unloaded, but there is a certain stress or range of stresses over which the bar has the same magnetic properties whether the load be on or off.

In Experiment I. the range of stress referred to in (iii.) is about 15 to 22 tons per square inch, the latter being the yield-point; and in Experiment III. it is about 10 to 15 tons per square inch, the latter being the yield-point. In Experiment II. the critical stress appears to coincide very nearly with the stress at the yield-point.

It is interesting to compare these results with those obtained by Joule. In his 'Scientific Papers,' pp. 253–256, Joule shows that an iron or steel bar elongates on being magnetized, but that if the bar is strained, the effect is reduced until, at a certain load, no effect is observed, and if the load be further increased, magnetization produces a shortening of the bar. These experiments were made with soft iron and steel wires.

Joule's experiments show that magnetization produces a minute elongation of a bar, those quoted above show that mechanical stress increases the magnetization of a bar, *i. e.* mechanical stress produces a change similar to that produced by magnetization. It would seem, then, that Joule's critical point of no elongation (+ or −) must be reached when the molecular elongation produced by the mechanical stress is the same as that which would be produced independently by a certain given intensity of magnetization. For at that point, the molecular elongation having already been produced by mechanical stress, the effect of the given magnetization would be nil, *i. e.* there would be no further elongation (+ or −). Now, taking an intensity of magnetization, denoted by 5 in Joule's experiments, we see that this produces an elongation in a 12 inch length of soft iron wire (0·25 in. diam.) of $\frac{4}{150,960}$ in.; assuming Young's modulus for the iron at 13×10^3 tons per square inch, the mechanical stress required to produce this elongation would be about 0·03 ton per square inch.

According to Joule's experiments the critical stress is at about 6 tons per square inch, *i. e.* in order to produce this molecular elongation by mechanical means we require an elongation of the bar 200 times as great as that which accompanies the same effect produced by magnetization.

We are thus dealing with two distinct kinds of elongations—firstly, that produced by a relative motion of the molecules; and secondly, that produced by a straining of the molecules themselves.

It is clear that if the atomic displacement should be permanent, there would be a permanent change in the physical and chemical properties of the iron; and as there certainly is a change produced by permanent set, it is highly probable that we must look to such a straining of the molecule to account for it.

Thus, taken in conjunction with Osmond's experiments, those quoted above would seem to show that the increase in the value of the limit of elastic resistance with permanent strain is due to a permanent straining of the individual molecules.

Experiment I.—Steel Bar.

Load.	Micrometer.	Time in seconds of 20 vibrations.	$\frac{N^2-x^2}{10}$.	Load.	Micrometer.	Time in seconds of 20 vibrations.	$\frac{N^2-x^2}{10}$.
0·0	1·417	24·9	218	0·0	1·418	18·9	389
0·1	1·417	24·85	219	5·4	1·457	18·7	398
0·2	1·417	25·0	217	0·0	1·418	18·7	398
0·4	1·418	24·8	221	0·0	1·418	18·65	400 †
0·0	1·417	24·85	219	5·6	1·458	18·65	400
0·6	1·420	24·75	221	0·0	1·418	18·6	403
0·8	1·420	24·6	224	5·8	1·463	18·7	398
1·0	1·422	24·55	225	0·0	1·420	18·5	407
0·0	1·417	24·9	219	6·0	1·465	18·3	416
1·2	1·424	24·1	234	0·0	1·420	18·5	407
1·4	1·425	23·7	243	6·2	1·465	18·35	414
1·6	1·426	23·35	250	0·0	1·420	18·4	412
1·8	1·427	22·4	273	6·4	1·467	18·2	421
2·0	1·428	21·8	289	0·0	1·421	18·55	405
0·0	1·417	22·6	268	6·6	1·469	18·3	416
0·0	1·417	22·5	271*	0·0	1·421	18·3	416
2·2	1·430	20·9	316	6·8	1·470	18·0	431
2·4	1·431	20·55	327	0·0	1·421	18·6	403
2·6	1·433	20·4	332	7·0 §			
2·8	1·434	20·3	336	0·0	1·455	18·8	394
3·0	1·436	19·85	352	7·0 §			
0·0	1·417	20·7	322	0·0	1·507	19·2	377
0·0	1·417	20·75	321 *	7·0 §			
3·2	1·437	19·6	361	0·0	1·538	19·4	369
3·4	1·438	19·5	365	0·0	1·538	19·5	365 ‡
3·6	1·439	19·65	359	7·7	2·095	17·95	433
3·8	1·440	19·6	361	0·0	2·036	25·35	210
4·0	1·442	19·45	367	8·0	2 178	16·5	515
0·0	1·417	20·05	344	0·0	2·112	25·05	216
0·0	1·417	19·9	350 *	8·3	2·323	16·8	497
4·2	1·444	19·1	381	0·0	2·248	25·45	209
0·0	1·417	19·75	355	8·6	2·455	16·65	506
4·4	1·446	19·15	379	0·0	2·374	25·7	204
0·0	1·417	19·45	367	9 0	2·692	17·05	482
4 6	1·451	19·1	381	0·0	2·616	26·4	193
0·0	1·418	19·5	365	9·3	2·981	16·8	497
4·8	1·452	18·8	394	0·0	2·893	26·35	194
0·0	1·418	18·9	389	10·0	……	17·4	462
0·0	1·418	18·9	389 †	0·0	……	27·5	177
5·0	1·453	18·85	391	10·5	……	17·35	465
0·0	1·418	19·05	383	0·0	……	28·0	170
5·2	1·455	18·65	400	10·87	……	18·1	426 ‖

* After 10″.
† After 5″.
‡ After 17 hours.
§ Load removed after small permanent set.
‖ After fracture.

Experiment III.—Wrought-iron Bar.

Load in tons.	n. Time in seconds of 20 vibrations.	$n^{-2} . 10^5$.
0·0	17·25	336
0·5	16·8	354
0·0	17·1	342
0·0	16·9	350*
0·7	15·25	430
1·0	14·05	507
0·0	14·7	463
1·3	13·05	587
1·6	13·15	578
0·0	13·5	549
2·0	12·95	596
0·0	12·95	596
2·2	13·1	583
0·0	13·4	557
2·4	12·8	610
0·0	13·2	574
2·7	13·25	570
0·0	12·85	606
3·0	13·0	592
0·0	13·3	565
3·12	12·75	615†
0·0	14·85	453
3·4	12·75	615
0·0	15·2	433
3·8	12·85	606
4·2	13·55	545
0·0	16·25	379
4·4	13·55	545
0·0	15·95	393

* After one hour.

† After the permanent set.

PROCEEDINGS

OF

THE PHYSICAL SOCIETY

OF LONDON.

JUNE 1890.

XLIV. *Twisted Strips.*

By Professor JOHN PERRY, *F.R.S.*[*]

IN a paper read at the British Association Meeting this year, and also at a former meeting of the Physical Society, I referred to the curious behaviour of a twisted strip. The twisted strip must not be confounded with what has been called the Ayrton and Perry Spring. It is a straight strip of metal to which a permanent twisted appearance has been given, a twisting moment being applied about the axis of the strip to give it the permanent twist, care being taken to prevent the axis of the strip from lateral motion. In fact the axis remains straight, and is the axis of all the spiral lines now formed by the lines which were originally straight and parallel to the axis. When an axial force is employed to elongate a twisted strip, a slight elongation takes place accompanied by a large relative rotation of the ends of the strip. The only simple hypothesis on which, as I told the Society on a former occasion, I could see my way to building a possible theory was to consider in any very short length of the strip any two spiral filaments equally distant from the axis as two threads in a bifilar suspension. Under the action of any axial force these two threads tend to produce untwisting, and the sum of all these untwisting moments is equal to the torsional rigidity of the cross section multiplied by the angle of untwist.

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[*] Read November 1, 1889.

The phenomena are so complicated that I never hoped to explain them all by this or any other hypothesis. I was seeking for a roughly correct theory only.

The breadth being b and thickness t, we may roughly assume that the section remains rectangular; and as b is always more than ten times t, we may take the torsional rigidity as $\frac{N}{3}bt^3$. If then θ is the angular amount of untwisting produced per unit length of the strip, the moment of torsional resistance is

$$\frac{N}{3}bt^3\theta.$$

Let w be the axial force. If ϕ is the twist of the strip per unit length, then for a filament whose horizontal section is $t\,.\,\delta x$, at the distance x from the axis (as t is small compared with b I take the filament as extending quite across the section), if p is the vertical tensile stress on the section, it is obvious that the part of the untwisting moment due to one filament is $tpx^2\phi\,.\,\delta x$; so that

$$\frac{N}{3}bt^3\theta = t\phi\int_{-\frac{1}{2}b}^{\frac{1}{2}b} px^2\,.\,dx\,; \quad . \quad . \quad . \quad . \quad (1)$$

also

$$w = t\int_{-\frac{1}{2}b}^{\frac{1}{2}b} p\,.\,dx. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

I explained on the former occasion that, for a working theory, it ought to be sufficient to take p constant over the section. Hence, from (2),

$$p = \frac{w}{bt}\,; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

and inserting this value in (1), we have

$$\frac{N}{3}bt^3\theta = \tfrac{1}{12}t\phi b^3\frac{w}{bt},$$

or

$$\theta = \frac{\phi b w}{4t^3 N}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

If θ is small in comparison with ϕ, it is allowable to take ϕ

as the twist of the strip when no axial load is acting—that is, its permanent twist.

Now I announced on the former occasion that careful experiments made upon large twisted steel strips of various dimensions did not by any means agree with my result. It is true that θ was proportional to w; but it seemed rather independent of ϕ, and instead of being proportional to b, θ seemed to get less as b was greater; and, again, instead of t^3 in the denominator some power of t between 2 and 3 ought to be taken.

Under these circumstances I asked members of the Society for assistance in obtaining a working theory, but hitherto I have not obtained any.

I wish now to announce that the experiments I referred to, made very carefully by Mr. Still, of the Finsbury College, under my direction, were not sufficiently exhaustive. Mr. C. E. Holland has lately been kind enough to make an experiment for me on a very small strip of platinum silver. He finds that when ϕ is large his results agree very closely with the former results; but as ϕ is made less and less, the law connecting θ and ϕ becomes more and more nearly what is given by (4). The experiments were carried out upon double-twisted strips. That is, a straight strip was taken with its ends fastened in a frame; it was caught in the middle and there a twisting-couple was applied, so that one half of it took a right-handed and the other a left-handed permanent twist. Beginning from almost the condition $\phi=0$, by giving more and more permanent twist to the strip and applying loads in each case, it was seen that, for a given axial load, θ was nearly proportional to ϕ at first, but as ϕ became greater and greater the increase of θ was less rapid, θ eventually remaining nearly constant, although ϕ was increased.

I now know that Mr. Still's experiments were carried out upon strips in which there existed so much permanent twist as to resemble the strips in the condition in which they were towards the end of Mr. Holland's twisting operations, and hence θ seemed independent of ϕ.

Now if, instead of taking p constant and using (3), we take p as getting less for filaments at greater distances from the axis, it is obvious that we shall arrive at a result which, although more complicated than (4), will represent much better the results of experiments.

Thus, as is probable enough, if the tensile strain in every filament is such as to keep a section at right angles to the axis parallel to itself, it is easy to show that, instead of p being constant, we must take

$$p=\frac{k}{t(1+x^2\phi^2)}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Using this in (2), we find

$$k=\frac{\phi w}{2\tan^{-1}\frac{1}{2}b\phi}.$$

Substituting (5) in (1) we find, on simplifying,

$$\theta=\frac{3w}{\mathrm{N}t^3}\left\{\frac{1}{2\tan^{-1}\frac{1}{2}b\phi}-\frac{1}{\phi b}\right\}. \quad . \quad . \quad . \quad (6)$$

It is obvious that, except when $b\phi$ is great, (6) becomes

$$\theta=\frac{w}{4\mathrm{N}t^3}\left\{\phi b-\frac{\phi^3b^3}{15}+\frac{\phi^5b^5}{115}\right\}, \quad . \quad . \quad . \quad (7)$$

which agrees with (4) for small values of ϕb.

And evidently from (6), as $b\phi$ gets greater and greater, θ reaches more and more nearly the limiting value,

$$\frac{3w}{\mathrm{N}t^3\pi}.$$

Making p any other likely function of $x\phi$ than that given in (5) willmerely modify the constants in (7).

Professor Ayrton and I have used these strips in weighing-machines, instruments in which forces require to be measured, and instruments in which small motions require to be magnified. We are now using the double-twisted strip of constant length, but with initial pull in it, as a thermometer and as a galvanometer. It is my opinion that it only requires to be better known to be largely used; and although I seem to have spent rather in vain a considerable amount of time in trying to get a working theory, I do not think the time has been really wasted. It is no mere curious puzzle, it is a problem of practical importance.

The two great difficulties are—(1) What is the law of p over the section? (2) To what extent is it wrong to regard the section as resisting untwisting with the moment

$$\frac{N}{3} bt^3\theta\,?$$

that is, to what extent has the original twisting given initial shear strains to the material?

XLV. *On a Carbon Deposit in a Blake Telephone Transmitter.* *By* F. B. Hawes[*].

(Abstract.)

The Author exhibited photographs of the interior portions of the transmitter on which the deposit had taken place. These portions consist of a metal diaphragm, a highly polished carbon button, and a platinum contact-piece carried by a German-silver spring placed between them. The diaphragm presented a mottled appearance, due to the deposit, but the part which had been behind the German-silver spring seemed comparatively clean. The deposits on the carbon button and German-silver spring were much less dense than that on the exposed parts of the diaphragm, and the space near the point of contact between the platinum and carbon was free from deposit. The deposit was fairly adherent, considerable rubbing being necessary to remove it, and, on examination under the microscope, particles of copper and metallic crystals could be seen. The author believes the deposit due to some kind of bombardment of carbon particles, but was unable to say why it should occur, or why the varnished diaphragm should receive the greater deposit, although it was further from the carbon than the German-silver spring.

Mr. C. V. Boys said the photographs reminded him of a phenomenon he observed some time ago on a glass sheet against which one terminal of a dry pile had been resting for some weeks. Just as on the carbon button, the glass near the point of contact was clean, and has a comet-shaped deposit formed around it. He could offer no explanation of the appearance.

[*] Read February 21, 1890.

XLVI. *On an Apparatus for the Distillation of Mercury in a Vacuum.*

By WYNDHAM R. DUNSTAN, *M.A.*, *F.C.S.*, *Professor of Chemistry to the Pharmaceutical Society of Great Britain, and* T. S. DYMOND, *F.I.C.*, *F.C.S.*[*]

[From the Research Laboratory of the Pharmaceutical Society.]

DISTILLATION of impure mercury constitutes the best method of removing foreign metals, and distillation in a vacuum is the only feasible plan of conducting the operation in the laboratory. It is doubtful whether mercury can be completely freed from zinc by distillation, and it is therefore safer to remove this metal, before distillation is resorted to, by agitating the mercury with warm concentrated hydrochloric acid.

Different forms of apparatus for distilling mercury in a vacuum have been described by Weinhold, Weber, Wright, and Clark. The action of these different pieces of apparatus is primarily dependent on the same principle; but that described by Clark (Phil. Mag. 1884, xvii. p. 24) is undoubtedly the most convenient and is the form which is ordinarily used. It possesses, however, several disadvantages. It is inconveniently long, being at least 5 feet in length, and needs a special place in the laboratory where it must be permanently fixed. Moreover the apparatus is only applicable when a comparatively large quantity of mercury is available, since at the conclusion of the operation there remains in the apparatus between one half and one quarter of a litre of undistilled mercury, the amount being chiefly dependent on the diameter of the distillation-tube. To this residue more mercury must be added before distillation can be recommenced.

The still we are about to describe does not entail these disadvantages. It is easy and inexpensive to construct, and when once started is automatic in its action. It is rather more than a metre long, and when required for use can immediately be clamped on a retort-stand and may be as readily dismounted again. The residue of undistilled mercury which remains in the apparatus is less than 20 cubic centimetres.

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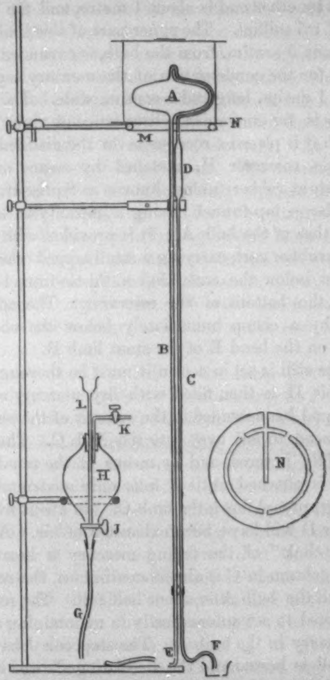
[*] Read March 7, 1890.

The apparatus is constructed in the following manner:—A bulb A is blown on one end of a piece of thick-walled soft glass tubing B, and is made to assume somewhat the shape of



an oblate spheroid, so that nearly the whole of the mercury may be distilled out of it. The other end of the tube has a small elbow-bend to admit of its connexion with the reservoir H by means of the stout rubber-tubing G. The diameter of the bore of this glass tube is 3 millim., and its length 1 metre, measured from the point at which it joins the bulb to the bend at the other end. To the summit of the bulb is sealed a piece of tubing which is bent round one side of the bulb and terminates in the limb C, which acts as a Sprengel pump. The length of the tube from the lower surface of the bulb to the bend at its other end is about 1 metre, and the bore has a diameter of 1·5 millim. The upper part of this limb, at a distance of about 5 centim. from the bulb, is expanded to form a chamber D for the condensation of the mercury vapour; this chamber is 1 decim. long and 1 centim. wide. The other end of the limb is for convenience bent twice at right angles F, and beneath it is placed a receptacle for the distilled mercury. The movable reservoir H, attached by means of at least 1 metre of stout rubber-tubing, known as "pressure" tubing, to E is a large tap-funnel having a capacity at least twice as great as that of the bulb A. It is provided with a tightly-fitting indiarubber cork carrying a small tapped tube K, which projects just below the cork, and a thistle-funnel L which extends to the bottom of the reservoir. The apparatus is supported by a clamp immediately below the chamber D, and it rests on the bend E of the stout limb B.

Before the still is set in action it must be thoroughly dried. The reservoir H is then filled with dry mercury and raised until the liquid has ascended to the summit of the bulb A, and has commenced to fall over into the limb C. The reservoir is now slightly lowered, and by means of the tap J the flow of mercury is checked until it falls only moderately fast (as in a Sprengel pump) down the limb C. In about five minutes the chamber D will have been exhausted of air. As soon as the sharp "click" of the falling mercury is heard and the descending column in C is almost continuous, the reservoir is lowered until the bulb A is about half full. The reservoir H is now adjusted to act automatically in maintaining a constant level of mercury in the bulb A. The stopcock J having been shut, the bulb is heated and the distillation allowed to proceed for about half an hour until the vapour-pressure of the mercury for the maximum temperature to which the bulb will be raised has been attained. Air is then sucked out of the tube K until bubbles pass up through the mercury, when the tap in K is instantly closed. The tube of the funnel L now contains no mercury, the pressure of the air and of the mercury in the reservoir being exactly equal to the atmospheric pressure acting through L. The level of the mercury in the reservoir must now be adjusted until the surface of the mercury in the bulb is distant from the bottom of the reservoir (above J) by the height of the barometric column less the vapour-pressure of mercury for the temperature at which distillation occurs. This temperature having been asçertained, the adjustment may be made after direct measurement of the mercurial column. It may also be effected perhaps more readily by raising the reservoir little by little until on momentarily opening the stopcock J air is observed to bubble through the mercury. As soon as this occurs, the stopcock J is left open. The mercury from the reservoir H will now be automatically supplied to the bulb A. As the mercury distils out of the bulb more rises from the reservoir to take its place, and air enters from the funnel L and bubbles through the mercury in the reservoir into the closed space above. Through this action of the reservoir the level of the mercury in the bulb will remain the same until the whole of the mercury contained in the reservoir has been distilled.

In order to heat the bulb A the tubes just below it are surrounded by the ring-burner M, which may be a circular piece of metal or glass tubing perforated at regular intervals with small holes; the flames proceeding from these perforations should be very small, and must on no account be allowed to impinge on the bulb or on either of the tubes. The bulb is now enclosed on all sides so that a hot air-chamber may result. This is best done by cutting a circular disk N, whose circumference is somewhat greater than that of the ring-burner, out of asbestos millboard, incisions being made to correspond with the tubes so that the disk may slide under the bulb and rest on the burner itself. On this disk as a base a cylinder of the same material, or of glass, is placed, the cylinder being wider and taller than the bulb. The cylinder is closed at the top by a rather larger and circular piece of asbestos millboard, in the centre of which a small round hole should be cut. By partially or entirely covering this hole with a small disk of millboard, it is easy to regulate the admission of air to the chamber. The cylinder is prepared from the millboard by thoroughly wetting it and rolling it round a bottle or wooden block of the proper size. When dry the cylinder is removed and secured by twisting a piece of thin copper wire round it. The bulb is thus heated in the air-chamber, and not by direct contact with the flames, and the bulb is effectually protected from the disastrous effects of draughts of cold air.

The temperature of the air-bath when distillation is proceeding ranges between 200° and 300° C. Since fresh mercury is automatically supplied to the bulb the still requires no attention, and may safely be left at work day and night provided that the gas pressure is almost constant. This may of course be ensured by fixing a gas-regulator in the air-bath. We have found, however, that an ordinary Sugg's dry governor introduced between the gas supply and the burner answers the purpose sufficiently well.

When the still is started for the first time the air-film on the glass is detached and passes away down the limb C. The distillation consists in continuous evaporation from the liquid surface unaccompanied by actual ebullition, or by the "bumping" and spirting which are so noticeable when mercury is distilled under the ordinary atmospheric pressure.

The rate of distillation is mainly dependent on the capacity of the bulb A. We have generally worked with a bulb rather smaller than that described above, because it is easier to blow it of sufficient thickness. As the still is automatic in its action and needs no attention the quick distillation of a large quantity of mercury is not a point of much importance. It has frequently happened that the distillation has come to an end during the night, both the reservoir and the bulb being discovered nearly if not quite empty. This, however, has occasioned no inconvenience; it is only necessary to introduce more mercury into the reservoir and from thence into the bulb. This can be done without extinguishing the burner. The stopcock J is closed and the tap K is opened. Mercury is then poured through the funnel L. The stopcock F is now opened very gradually, so that cold mercury may not suddenly rush against the hot glass, until the bulb is half full, when the stopcock J is closed again. A piece of rubber-tubing is now attached to K, and air is sucked out until bubbles begin to rise through the mercury in the reservoir, when K is shut; after the lapse of about half an hour the stopcock J is opened. When a distillation has been finished and the apparatus is not further required for the present, it may be left standing with the stopcock J closed, and the end F dipping into mercury or otherwise sealed; it can then be started again at any moment. To empty the still, F is closed and the apparatus is carefully tilted so that the mercury remaining in C may flow into the chamber D. If now the end F be cautiously unclosed air will enter the apparatus through the empty limb C.

When this apparatus is at work in a dark room the remarkable phenomenon of the "flashing" of mercury is observed. It is seen in the condensation-chamber D. As the mercury liquefies flashes of green light are produced, which exhibit the spectrum of mercury. It would seem that this is an electrical effect arising from friction between mercury and glass. We have noticed that it is enormously intensified when one of the dischargers of a Wimshurst machine is brought near the chamber D. Under these circumstances the entire chamber becomes illuminated with a magnificent green light.

The apparatus may also be conveniently employed for observing the electrical discharge through mercury-vapour at different temperatures. For this purpose the ascending column of mercury in B and the descending column of mercury in C are used as electrodes. One terminal from the coil dips into the mercury in the reservoir H, the other into the distilled mercury which has collected in the receptacle under F, and below the surface of which the extremity of the tube C is immersed.

We are now making observations on the character of the discharge which occurs under these conditions, in the absence of any foreign metal.

Prof. Thompson said he devised a simple form of distilling-apparatus some time ago which answered fairly well, and could be made by any amateur glass-worker. It consisted of a double barometer, one leg of which was of small bore, so as to act as a Sprengel-tube. The rising part of the bend at the top of the larger tube was expanded, and served as the evaporating-chamber, below which a burner was placed.

The President asked why Clark's apparatus is made so lengthy.

In reply to this question Mr. Boys said that, as the fall-tube goes down within the rising one, the mercury near the top of the latter is heated by the condensing mercury (thus economising gas), and hence condensation does not take place until the vapour has passed a considerable distance down the fall-tube.

XLVII. *The Theory of Osmotic Pressure and its bearing on the Nature of Solutions.* *By* SPENCER UMFREVILLE PICKERING, *M.A.*[*]

NO one can doubt the mathematical correctness of the conclusions which Arrhenius, van't Hoff, Ostwald, and others draw from the premisses with which they start in their arguments respecting osmotic pressure, nor can we doubt the value of connecting numerous actions with one and the same cause, or that there are a large number of instances in which the observed facts are in substantial agreement with their conclusions. But we may, I think, legitimately doubt whether the premisses of the arguments are sound, whether the conclusions harmonize as well as they should with experimental data, whether the theory is more than a mathematical exercise, or more than a convenient working hypothesis of a rough character, instead of being, as its supporters maintain, an hypothesis established so firmly that we may build upon it a physical theory of solution.

The direct measurement of osmotic pressure has been made in but a few cases, and those cases are ones in which the substances examined are eminently unfitted for showing the presence of any chemical action which may be present. Of the phenomena correlated with osmotic pressure, the lowering of the freezing-point of a solvent by the addition of foreign substances is the one which has received the greatest attention, thanks to Raoult's classical work, and is the one which forms the main support of the theory.

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[*] Read March 7, 1890.

In examining the experimental facts the following questions must be asked, and if the theory is correct they must be answered in the affirmative.

(1) Is the molecular depression—using this term to mean the lowering of the freezing-point produced by one foreign molecule on 100 *molecules* (not *parts by weight*) of the solvent—a constant, whatever the foreign substance is, provided that the amount of the latter is not greater than that which would give a pressure equal to that produced by it in the gaseous state at the same temperature?

(2) Are such deviations from regularity, as are observed in exceptional cases, in the direction in which they should be according to the theory?

(3) Is the depression a constant whatever the solvent is?

(4) Is the depression constant when the proportions of the solvent and dissolved substance are varied within the limits mentioned in (1)?

(5) When stronger solutions are taken, is the deviation from regularity in the direction in which it should be according to the theory?

(6) Is the deviation always in the same direction, and always regular?

I believe that I can show that the answer to every one of these questions is an unqualified No.

The main facts of the case as elucidated by Raoult's work are that, when various substances are dissolved in

Benzene,
Nitrobenzene,
Ethylene dibromide,
Formic acid,
or Acetic acid,

the molecular depression is about 0°·63, though there are a few substances in each case which give an abnormal depression of about half this amount.

With water the molecular depression is generally 2°·06, but in many cases approximates to 2°·61, values which Raoult takes to be three and four times 0°·63 (1°·89 and 2°·52) respectively. Abnormally low values are in the same proportion to the normal ones as with other solvents; they are about 1°·03 (half of 2°·06), and are obtained with some inorganic substances, and with the majority of organic substances at present investigated.

All these determinations apply to solutions containing about 1 molecule to 100 molecules of the solvent, and are, therefore, three or four times stronger than they should be in order to be comparable with gases under ordinary conditions. We shall have to decide whether the discrepancies which they show are such as could be reasonably attributed to increasing the pressure from one to three or four atmospheres.

Influence of the Nature of the Dissolved Substance.

The half values obtained in many cases are explained by the duplication of molecules, owing to the solutions not being sufficiently dilute; that is, an additional pressure of two or three atmospheres will cause the vapour-densities of hydrochloric acid *, methyl, ethyl, amyl, and butyl alcohols †, benzol, and many other substances to double themselves (assuming that they retain their gaseous condition). This may be so, but direct determinations cannot be, or have not been, made.

Leaving these abnormal values for the present, we find that the normal ones exhibit the following variations:—

Solvent.	Number of instances investigated.	Variation in the value of the depression ‡.
Acetic acid	57	30 per cent.
Formic acid	9	14 ,,
Benzene	41	12 ,,
Nitrobenzene	13	7 ,,
Ethylene dibromide	5	2 ,,
Water	59	60§ ,,
,, (with organic substances)	29	40 ,,

These variations appear somewhat too large to be attributed to the fact that we are working at two or three more atmospheres of pressure than we should be.

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* Solvent, Acetic acid (Raoult, *Ann. Chim. Phys.* [6] xi. p. 72, from which paper this and other values quoted here are taken).

† Solvents, Benzene, &c.

‡ Percentage on the lowest value.

§ Omitting borax, where the value is exceptionally high.

It is noticeable that the organic solvents, except acetic acid, give comparatively constant results, but that, as soon as we come to the inorganic solvent water (which we know shows a much greater tendency to form "molecular" compounds than the other liquids), we get a much larger variation, and the following values * will justify the assumption that when other solvents of a similar nature are examined, still greater variations will be observed.

Solvent.	Dissolved substance.	Depression†.	Variation†.
			Per cent.
Tetrahydrate of sulphuric acid.	Water.	0°·08	100
Do.	Sulphuric acid.	0·04	
Monohydrate of sulphuric acid.	Water.	0·02	100
Do.	Sulphuric acid.	0·01	
Sulphuric acid.	Water.	0·07	500
Do.	Sulphuric anhydride.	0·42	
Hexahydrate of calcium chloride.	Water.	0·003	233
Do.	Anhyd. calc. chlor.	0·010	
Tetrahydrate of calcium nitrate.	Water.	0·007	100
Do.	Anhyd. calc. nitrate.	0·015	

As an instance of a similar character we may quote Heycock and Neville's determinations of the depression of the freezing-point of sodium by various metals ‡.

Metal added.	Molec. depression.
Gold	4°·5
Thallium	4·3
Mercury	4·5
Cadmium	3·0
Potassium	3·6
Lithium	1·3
Lead	4·6
Indium	3·5

The variation here is 260 per cent.

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* Chem. Soc. Trans. 1890. The values selected refer to solutions of the same strength as those used by Raoult. The results with calcium nitrate and chloride are as yet unpublished.

† Most of these values are but approximations.

‡ Chem. Soc. Trans. 1889, p. 667. The values refer to solutions containing as near as possible one atom of the foreign metal to 100 Na.

If such variations are not too great to be attributed to an excess of two or three atmospheres' pressure (and I certainly think they are), they are undoubtedly too great to permit of a theory being founded on the assumption that they are constant.

It may be noted also that Heycock and Neville's determinations with indium, gold, and thallium extended to solutions as dilute as the gases from these metals would be (if their molecules are diatomic), and that even with these "ideal" solutions the variation amounted to 60 per cent.; had lithium and cadmium been similarly investigated we should, no doubt, have found as great a variation as that quoted above with the stronger solutions.

Direction of the Deviations with Dissolved Substances which give Abnormal Values.

The abnormally low values (*i. e.* half values) which some dissolved substances give are attributed, as has been mentioned, to the polymerization of their acting molecules, and the abnormally high values which some solvents give are, in the same way, attributed to a polymerization of the solvent molecules. This view is accepted by both the supporters and the opponents of the osmotic pressure theory.

In the case of dissolved substances which give abnormally high values (*i. e.* double values), the supporters of the theory hold that these substances are dissociated into their ions, and in this view their opponents cannot agree with them.

The first point to settle, is whether there are any abnormally high values (given by dissolved substances), and it appears to me that there certainly are none.

In the case of the five organic solvents examined, the substances giving the higher and lower values are in the proportion of 100 to 16, and here there can be (and is) no question as to which are the normal ones; even if in the case of water these proportions were reversed, it would be inconsistent to term the lower values the normal ones; but the proportions are actually not reversed, the substances giving high values being to those which give the lower ones in the proportion of 100 to 76. I fail to see therefore what grounds there are for saying that there are any abnormally high (double) values at all, or what need there is for calling in such a theory as that of dissociation into ions[*].

Indeed, there is a distinct difficulty, if not impossibility, in calling the lower values (1°·03) the normal ones; for the normal value with other liquids is 0°·63, and the excess in the case of water could only be explained by the water molecules being more complex than those of other liquids in the proportion of 1·03 to 0·63; in other words, that the water molecule is $1\frac{1}{2}$ H_2O, and that the atomic theory is wrong.

The theory of dissociation into ions is, for many reasons, inacceptable to most chemists, and its application to the results at present under discussion affords, I think, some of the strongest arguments against its acceptance.

If the higher values in the case of water are due to dissociation, we must conclude that all the stronger acids (*e.g.* hydrochloric, sulphuric, and nitric acids) are nearly entirely dissociated, whereas the weaker ones (*e.g.* hydrocyanic, sulphurous, and boric acids) are not so; and similar conclusions must be drawn respecting the alkalies and salts. In fact, we shall be driven to conclude that the more stable the compound is the more easily is it dissociated, and that too by an agent, water, which has, *ex hypothesi*, no action beyond the diluting effect of its mass. Is it possible to maintain that sulphurous acid *exists* intact in solution, while sulphuric acid, which is formed from it and molecular oxygen, with the evolution of 55,000 cal.[†], and which must, therefore, be by so much the stabler body of the two, is entirely dissociated?

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[*] Van't Hoff's statement as to Raoult's position respecting the high and low values of substances in water is certainly remarkable; he says (Phil. Mag. 1888, xxvi. p. 99), "Raoult did not discover the existence of so-called normal [meaning small] molecular depression of freezing-point and lowering of vapour-pressure until he investigated organic compounds; their behaviour is almost without exception regular." As a matter of fact, Raoult investigated organic substances *first* (*Ann. Chim. Phys.* 1883 [5] xxviii. p. 133), and when he *subsequently* (*ibid.* 1884 [6] xi. p. 81) found that inorganic substances gave higher values, he did not hesitate to call these latter the normal ones.

If we reverse the application of the word normal, we shall have to admit that of the 150 substances examined in water, not one gave an abnormally low value. A most improbable admission.

[†] If atomic oxygen were taken, as it should be, the heat evolved would be much greater.

Is it possible to maintain that hydrochloric acid is entirely dissociated at a pressure of three or four atmospheres when we know as a fact that it can exist undissociated at a pressure of only one atmosphere [*] ?

Indeed, it appears to me that if we acknowledge such a dissociation we must deny the principle of the conservation of energy. Thus, to take one instance out of many, the molecules of hydrogen and chlorine must [†] be formed from their constituent atoms with a considerable development of heat, say $+x$ and $+y$ cal. respectively, and they react with each other to form 2HCl with a further development of 44,000 cal. ; if this 2HCl, when dissolved in water, be entirely dissociated into its atoms, it must *absorb* $44,000 + x + y$ cal., whereas, as a matter of fact, it *evolves* 34,630 cal. : hence there has been a creation of $(78,630 + x + y)$ cal. out of nothing, and that, too, through the agency of the water, which is assumed to have no action at all.

Influence of the Nature of the Solvent.

It is found that the nature of the solvent, instead of being without influence on the results, as it should be according to the osmotic-pressure theory, is one of the main factors in determining them.

Thus, for instance, sulphuric and hydrochloric acids give the normal high values when dissolved in water, but solutions of the *same* strength in acetic acid show the abnormal half-values : formic and benzoic acids, methyl, ethyl, butyl, and amyl alcohols, as well as phenol, all give normal depressions with acetic acid, but abnormal ones with benzene. Many other instances of a similar character may be selected from Raoult's data, but none of them show the influence of the nature of the solvent in such a forcible manner as the following :—

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[*] In acetic acid hydrochloric acid is, according even to the supporters of the theory, not only not dissociated into its ions, but even not dissociated into its fundamental molecules, it being present as 2HCl.

[†] I think that we may say "must." If they were formed with absorption of heat it would mean that the atoms repel each other, and such atoms would, therefore, never combine. When we call a molecule endothermic, we merely mean that it is formed from its constituent atoms with a smaller development of heat than some other compound of the same atoms, not that it is formed from these atoms with an *absorption* of heat.

	Molec. Depress.
H_2O dissolved in 100 H_2SO_4	1°·072
" " 100 ($H_2SO_4H_2O$) . . .	0·02
" " 100 (H_2SO_4 $4H_2O$)	0·08
" " 100 ($CaCl_2$ $6H_2O$)	0·003
" " 100 ($Ca(NO_3)_2$ $4H_2O$) . .	0·007
" " 100 (CH_3COOH) . . .	0·55

The variation of this constant (?) calculated on the smallest value amounts to 35,600 per cent. Again,

H_2SO_4 dissolved in 100 H_2O	2·15
" " 100 ($H_2SO_4H_2O$) . . .	0·01
" " 100 (H_2SO_4 $4H_2O$) . .	0·04
" " 100 (CH_3COOH) . .	0·31

Extreme variation 21,400 per cent. Similarly,

$CaCl_2$ dissolved in 100 H_2O	2·773
" " 100 ($CaCl_2$ $6H_2O$) . . .	0·010

Variation 27,600 per cent.; and

$Ca(NO_3)_2$ dissolved in 100 H_2O	2·50
" " 100 $Ca(NO_3)_2$ $4H_2O$.	0·015

Variation 16,600 per cent.

Such numbers can certainly not be accepted as constants, and the values are certainly largely dependent on the nature of the solvent*.

In the present instance the variation cannot always be taken as a proof of the incorrectness of the osmotic-pressure theory, for it may be perfectly legitimate to attribute it sometimes to the various complexity of the molecules of the solvent. But we cannot, however, do so in all cases, as for instance that of formic &c. acids in benzene and acetic acid, where the two solvents have been proved to act normally with the majority of dissolved substances. Yet, in any case, the absence of constancy means the absence of what would have been a strong argument in favour of the theory, and this absence must be felt all the more because there appears to be no simple numerical relationship between the very different values given above.

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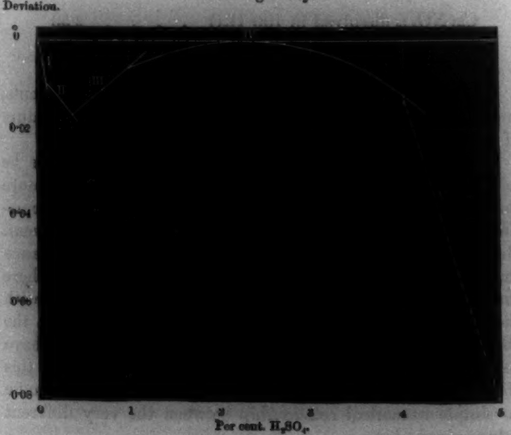
* Combining the variations due to different solvents with those due to different dissolved substances, we get a variation of 153,000 per cent. (4°·6 to 0°·003).

Influence of Proportions taken with very Weak Solutions.

My own recently published results (Chem. Soc. Trans. 1890, p. 331) with sulphuric-acid solutions form, I believe, the only series of determinations as yet made which would, from their number and precision, be at all likely to settle whether the molecular depression is really constant or not with very weak solutions; and the results here can leave little doubt but that it is not so.

Between 0 and 9 per cent. solutions the results are represented by what would with rough determinations appear to be a single line of typical straightness. The deviations from straightness in regions below 4 per cent. (about 0·8 H_2SO_4 to 100 H_2O) are less than 0°·02, and would, therefore, not have been revealed by determinations such as have been made by other investigators. From 4 per cent. to 0 the determinations (each being the mean of several observations) which I made

Deviation of the Freezing-points of weak solutions of Sulphuric Acid from regularity.



show mean error of 0°·0008 at most, and the number of points determined is about 80; and the results obtained leave no doubt but that the apparently straight line is really composed of four distinct lines, three of which are straight, meeting at (generally) well marked angles at the percentages of 1·0, 0·35, and 0·07 respectively.

The relative magnitude of these changes is best illustrated by drawing a straight line from the freezing-point of water to that of a 2·5 per cent. solution (H_2SO_4 to 200 H_2O; this was selected merely because at this percentage the diagram attains the highest relative point) and plotting out the deviations of the observed freezing-points from this line. The accompanying figure illustrates the result. The deviation from straightness amounts to at least 20 times the mean experimental error, and 28 per cent. of the total depression. Even if we draw our straight line from a solution of 0·44 (·08 H_2SO_4 to 100 H_2O) per cent. strength—in which the sulphuric acid would be as much expanded as it would when a gas at ordinary pressures—the aberration, at 0·7 per cent., amounts to 0°·008, or 20 per cent. of the total depression.

The molecular depression calculated from these results, together with others obtained with stronger solutions, are given in the following table:—

Depression of the Freezing-point of Water produced by the addition of Sulphuric Acid.

Per cent. H_2SO_4.	Mols. H_2SO_4 to 100 H_2O.	F.-P.	Molecular depression,
0·068	0·0125	– 0°·0369	2°·95[*]
0·3618	0·0667	– 0·1580	2·37
0·44	0·0811	– 0·1874	2·31
1·057	0·1961	– 0·4214	2·15
2·500	0·4708	– 0·9825	2·09
4·021	0·7692	– 1·600	2·08
6·000	1·1855	– 2·520	2·15
8·585	1·724	– 3·456	2·00
18·492	4·167	–11·83	2·84
29·526	7·692	–34·00	3·74
37·701	11·111	–72·63	6·54

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[*] With still weaker solutions the molecular depression is somewhat greater: with a 0·005 per cent. solution (·0009 H_2SO_4 to 100H_2O) the value found was 3°·4, but the total depression here is so small that I have not included it or similar values in the table.

The depression with solutions weaker than H_2SO_4 to 100 H_2O varies between 2°·08 and 2°·95, 42 per cent., and even between 0·44 and 0 per cent. it varies from 2°·31 to 2°·95, a variation of 28 per cent., numbers which can give no grounds whatever for the statement that the depression, even with these very weak solutions, is independent of the strength.

The only other determinations which, so far as I know, exist with solutions containing less than one dissolved molecule to 100 of the solvent, are those of Heycock and Neville (*loc. sup. cit.*) with certain alloys. Selections from those series which were most fully investigated supply the following data:—

Solvent.	Dissolved substance.	Molecular depression.
100 Na	0·114 Au	° 5·45
,,	0·296 ,,	4·74
,,	0·966 ,,	4·54
,,	0·097 Cd	3·92
,,	0·391 ,,	3·71
,,	0·648 ,,	3·17*

Direction of the Deviations from Constancy with Stronger Solutions.

According to the theory of osmotic pressure the proportionality between pressure (lowering of freezing-point) and amount of substance dissolved will hold good, as has been remarked, only for very weak solutions. With stronger solutions the dissolved molecules will be brought within the sphere of each other's attraction and will, therefore, exert an abnormally small pressure; that is, they will lower the freezing-point of the solvent to an abnormally small extent †.

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* With thallium, mercury, sodium, and indium, the depression was more constant, but the data were less complete.

† The same will hold good if we attribute dissolution to the balancing of the physical attractions of (1) solvent for solvent molecules, (2) dissolved for dissolved molecules, and (3) solvent for dissolved molecules. The stronger the solution the greater will be (2), and, consequently, the less will (1) be counterbalanced.

But in every case at present known exactly the reverse is the case. Stronger solutions exhibit an abnormally *large* depression, and the freezing-points fall at an increasing instead of a diminishing rate. The table given above illustrates to what extent this takes place in the case of solutions of sulphuric acid in water, the molecular depression increasing from $2^{\circ}{\cdot}08$ to $6^{\circ}{\cdot}54$ (213 per cent.), and the examination of six other curves from the sulphuric-acid series, and two of a series with calcium nitrate, supplies us with eight other instances of a similar character. The results may be summarized thus :—

Solvent.	Dissolved substance.	Molec. depression.	Increase.
100 H_2SO_4	1 to 33 H_2O	$1^{\circ}{\cdot}072$ to $1^{\circ}{\cdot}253$	17 per cent.
100 H_2SO_4	1 to 5·8 SO_3	0·422 to 0·634	50 „
100 $(H_2SO_4H_2O)$	1 to 50 H_2O	0·02 to 0·31	1,450 „
100 $(H_2SO_4H_2O)$	1 to 50 H_2SO_4	0·01 to 0·197	1,870 „
100 $(H_2SO_4 4H_2O)$	1 to 400 H_2O[*]	0·078 to 0·108	39 „
100 $(H_2SO_4 4H_2O)$	5 to 60 H_2SO_4[†]	0·27 to 0·42	55 „
100$(Ca(NO_3)_2 4H_2O)$	1 to 150 H_2O	0·007 to 0·326	45,600 „
100$(Ca(NO_3)_4 4H_2O)$	1 to 33$Ca(NO_3)_2$	0·015 to 0·179	11,000 „

It is true that these results have only recently been placed at the disposal of the supporters of the osmotic-pressure theory, but it is otherwise with the following values in the case of alcohol (Raoult, *Compt. Rend.* xc. p. 863) and calcium chloride (Hammerl, *Wien. Sitzungsber.* lxxviii. p. 59)[‡]:—

Solvent.	Dissolved substance.	Molec. depression.	Increase.
100 H_2O	1 to 7 $CaCl_2$	$2^{\circ}{\cdot}773$ to $6^{\circ}{\cdot}9$	150 per cent.
100 $(CaCl_2 6H_2O)$	1 to 800 H_2O	0·003 to 0·099	3200 „
100 H_2O	1 to 13 C_2H_6O	1·11 to 1·18	6 „

The increase in the case of alcohol is small, but still there is an increase, and not a decrease; while in the case of water in the hydrate of calcium chloride the increase is almost the greatest at present known.

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[*] With 60 H_2O the depression is $0^{\circ}{\cdot}067$, so that it first decreases and then increases.

[†] With 1 mol. H_2SO_4 it is $0^{\circ}{\cdot}40$.

[‡] In each case I give the values as determined from my own results, but these are in good accord with those referred to above.

Irregularity of the Deviations with Weak Solutions.

That the deviations from constancy are not regular, and are not even always in the same direction, will be seen on referring to the results with sulphuric acid dissolved in water (p. 363); the depression in the freezing-point of the tetrahydrate, both by water and sulphuric acid, supplying two other instances of a similar character.

Since every fact which can be used to test whether the theory of osmotic pressure is a true explanation of the nature and behaviour of solutions, either fails to give any evidence in its favour, or else gives evidence directly opposed to it—evidence, often, of the strongest possible character—this theory can certainly not yet be regarded as established.

XLVIII. Dr. S. P. Thompson described "*Bertrand's Refractometer*," and exhibited the capabilities of the instrument before the Society[*].

(Abstract.)

Its action depends on total reflexion. The refractometer consists of an hemisphere of glass about 8 millim. diameter set at the end of a tube, the plane face being outwards and inclined at about 30° with the axis. One side of the convex surface of the hemisphere is illuminated through a piece of ground glass set about perpendicular to the plane face. The hemisphere is viewed through an eyepiece focused on a scale divided to tenths of millimetres, placed within the tube. The instrument is particularly useful for mineralogical specimens and liquids. The procedure in the latter case is to smear a film of the liquid over the plane face of the hemisphere, and, by looking through the eyepiece, determine the scale-reading of the line which separates the light and darker portions of the field. A reference to a calibration-table gives the refractive index.

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[*] Read March 7, 1890.

In experimenting with solids, a thin film of very dense liquid (supplied with the instrument) is placed between the specimen and the glass, and the procedure is then as above. The refractive index of opaque solids can be determined in this way. In using the instrument for minerals, great care must be taken not to scratch the glass. The handiness of the refractometer and its perfect portability (its dimensions being about 5 centim. long by 2½ centim. diameter) are great recommendations.

Mr. Blakesley asked to what accuracy the scale could be read, and whether the sensitiveness of the instrument was at all comparable with that of other methods.

Prof. Dunstan inquired if it could be used with volatile liquids.

In reply, Dr. Thompson said that with non-homogeneous light the scale could be read to one division, but with a sodium-flame one tenth of a division could be estimated. For volatile liquids a drop may be used instead of a film, or the evaporation of a thick film may be retarded by a cover-glass.

XLIX. *The* Villari *Critical Points of Nickel and Iron*[*].

By HERBERT TOMLINSON, *F.R.S.*[†]

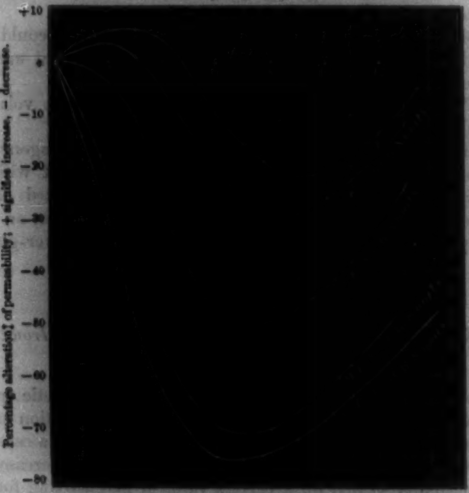
WE owe to Villari the discovery that the magnetic permeability of iron is increased by longitudinal traction provided that the magnetizing force does not exceed a certain limit, but beyond this limit the traction produces decrease of permeability so that for a certain value of magnetizing force, known as the Villari Critical Point, longitudinal traction has no effect in either direction. Several experimenters have

[*] Read March 21, 1890.

[†] The author begs to acknowledge with thanks the assistance which he has received in this investigation from the Elizabeth Thompson Science Fund, U.S.A.

verified and extended Villari's discovery, notably Sir William Thomson and Prof. Ewing, but, as far as the author is aware, no observer has been as yet able to find a similar critical point for nickel. If, however, we confine ourselves to the consideration of the *temporary* magnetization*, a Villari critical point in nickel can be detected with comparative ease. In the present investigation the ballistic method of observation has been employed, the arrangements of which have been already described†. The nickel wire used contains nearly 98 per cent. of nickel and only 0·7 per cent. of iron; the iron wire

Fig. 1.—Iron.



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* The magnetization which disappears on the removal of the magnetizing force.

† Phil. Mag. xxv. p. 372.

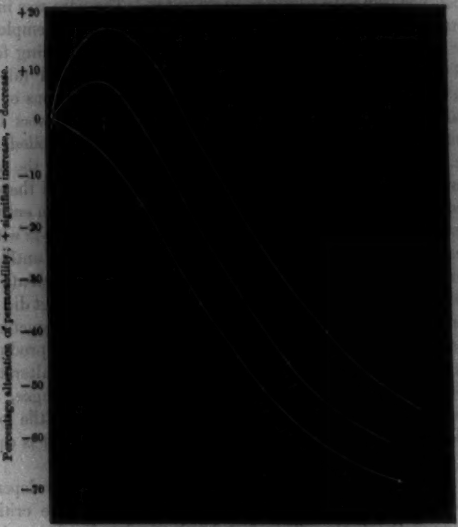
‡ If P_0 and P_n represent respectively the permeability with no load and a load of *n* kilos, the percentage alteration of permeability is taken to be $\frac{100(P_0-P_n)}{P_0}$ or $\frac{100(P_n-P_0)}{P_0}$ according as the permeability is decreased or increased by loading.

is also, judging from its small specific resistance, nearly pure. The mode of conducting the experiments may be illustrated by the aid of fig. 1; this figure refers to a piece of well-annealed iron wire 1 millim. in diameter. The wire was in the first instance loaded with 12 kilos (the highest load employed in the experiment), and was subjected to a magnetizing force of 15·5 C.G.S. units which was alternately applied (always in the same direction) and removed until the deflexions of the ballistic galvanometer became constant. The load was then entirely removed, and the same force was again applied and removed as before. A load of 2 kilos was now put on the wire, and the same operations were repeated with this and the subsequent loads up to 12 kilos. Next the load was again entirely removed, and a similar series of observations was made with a magnetizing force of 10·5 C.G.S. units, and so on until the force had diminished to 2·8. The entire set of experiments was then repeated with ascending forces up to 15·5; this set did not sensibly differ from the one taken with descending magnetizing forces. As might be expected, each change of load produced an alteration in the *residual* magnetization, but this alteration does not concern us, and only the *temporary* changes, *i.e.* those which take place after repeated application of the force and when the deflexion on closing the battery-circuit is equal to that on opening it, are recorded in the figure.

It may be noticed that the Villari critical point depends upon the load, the greater the load the smaller the critical value of the force. This fact has been already observed by Sir William Thomson, but for a given load the Villari critical point is *much lower* when only the *temporary* magnetization is taken into account than when, as in Sir W. Thomson's experiments, the *whole* magnetization is observed. It occurred therefore to the author that though it might not be possible to reach the critical force for nickel as far as the total magnetization is concerned, such a value could nevertheless be found for *temporary* magnetization, and this has proved to be the case.

Fig. 2 refers to a nickel wire of ·8 millim. diameter, which shows Villari critical points quite as striking as those of iron, but with this difference, that the critical value is *greater the* *greater the load*, and for a given load is much greater in nickel than in iron.

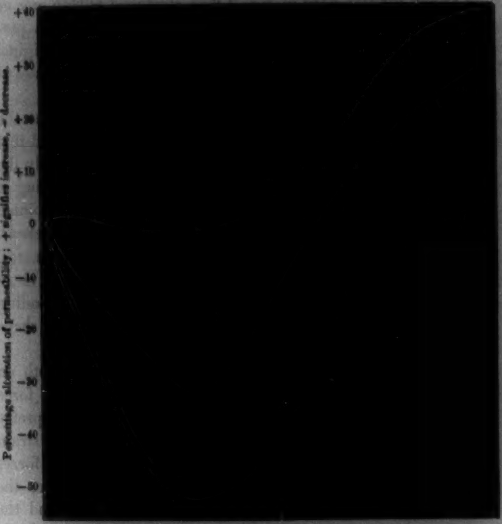
Fig. 2.—Nickel.



With iron wire the curves may, and do, sometimes cut the load line *twice*, so that for a given value of magnetizing force there may be *two* loads which have no effect on the temporary magnetization. In fig. 1 none of the curves *actually* cut the load-line a second time, because the loading was not carried quite far enough; but with subsequent experiments made with the same wire and with the same loads at a temperature of 200° C. the two points of cutting were easily obtained, within certain limits of magnetizing force, whilst the next figure shows that with greater loads the second point can be reached at the ordinary temperature of the room.

Fig. 3 illustrates the effect of loading on an iron wire of ·077 millim. diameter, which in the first instance was well annealed, but was subsequently loaded to very nearly its breaking-point, and was in consequence permanently

Fig. 3.—Iron*.



lengthened by about 10 per cent., whereas the wire referred to in fig. 1 was not loaded sufficiently to produce any sensible permanent extension. For a magnetizing force of 3 C.G.S. units the curve cuts the load-line twice, but for higher forces the first cutting-point disappears and we have only the second one.

The maximum decrease of permeability is greater the greater the magnetizing force up to a certain limit of the latter, which limit other experiments (not illustrated by the figure) proved to be about 16 C.G.S. units. Beyond this limit the maximum decrease begins to become slowly less, as is shown by the dotted curve of the figure for which the magnetizing force was 73.

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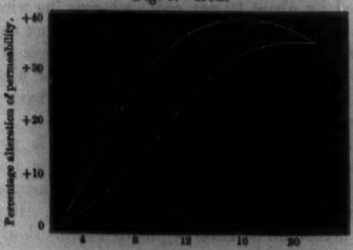
* In this experiment a load of 2 kilos was left permanently on the wire, so that the initial load is 2 instead of 0.

The curves for nickel can never be made to cut the load-line twice, for the wire of fig. 2 was stretched within one or two per cent. of its breaking-point, suffering thereby a permanent elongation of about five per cent. of its original length. The curves, however, show a *maximum* decrease of permeability with a load of about 24 kilos.

The observations hitherto recorded were only made at the different stages of *loading*, but subsequent ones showed that with nickel the readings on *unloading* were very nearly the same as those on *loading*, there being very little lagging of mechanical strain behind stress. Prof. Ewing has pointed out also that nickel exhibits very little *magnetic* hysteresis. With iron there is a very sensible lagging of mechanical strain behind stress, but the only effect of taking a mean between the observed changes on loading and unloading would have been to make the curves more parabolic in shape and not to alter their main character.

For the next two figures observations were made both on loading and unloading, and though the magnetizing force employed was here very minute, the curves show pretty nearly the same amount of lagging of mechanical strain behind stress which is experienced when much higher forces are used. For these very minute forces, of course, the apparatus had to be made much more sensitive, and the number of turns in the secondary coils was very great. Fig. 4 refers to the same kind of wire as that used in the first

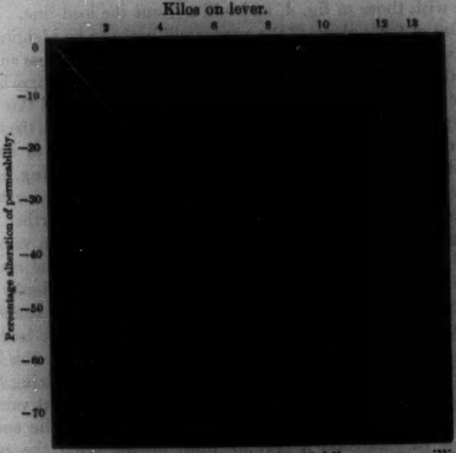
Fig. 4.—Iron.



experiment. The wire in this case was, however, previously loaded to nearly its breaking-point. If the mean be taken of the percentage alterations of permeability on loading and unloading, the curve becomes exactly parabolic, with its axis vertical and its vertex at the point where the vertical from the load 16 kilos. cuts the curve. Both the loading and unloading curves lie entirely *above* the load-line, and there is no point of cutting this line.

Fig. 5 deals with a nickel wire of the same degree of purity as the first but 1½ millim. in diameter. The stress was carried by the aid of a lever to very nearly the breaking-point of the wire. There is in this case no perceptible lagging of mechanical strain behind stress, the curve on loading being identical with that on unloading. With nickel the curve lies entirely *below* the load-line and never cuts the line.

Fig. 5.—Nickel.



1 kilo on the lever represents a stress of 2·68 kilos per sq. millim.

In figures 4 and 5 the curves are described as having been obtained with a magnetizing force of 0·03 C.G.S. units; but other experiments showed almost precisely the same curves to be obtained for any force between 0·03 and 0·3 C.G.S. units. Between these limits of force the *whole* of the magnetization is practically *temporary* provided the wires have been stretched to the extent these were.

From what has been said and from the figures it is easy to see what would be the general character of the curves, showing the relation between change of temporary permeability and change of load for any magnetizing force whatever. If a piece of iron wire be tested with loads nearly up to the breaking-point, then for magnetizing forces ranging from 0 to somewhere about ·3 C.G.S. units the curves will be *identical* with those of fig. 4, and will not cut the load-line. For a value of force above ·3 the curves will cease to be identical, the maximum increase of permeability will become less and less as the force increases until for a certain force the curve begins to cut the load-line. As the force increases beyond this value the point of cutting approaches nearer and nearer to the origin of coordinates, and the curve begins eventually to cut the load-line in two points. The first point of cutting, as the force increases still further, gets nearer and nearer to the origin, whilst the second point gets further and further away from it; and when the magnetizing force reaches a value but little over 3 C.G.S. units, the first point vanishes and the second point only remains. Finally, with a sufficiently high force it would seem that the second point also cannot be reached and the curve now lies *entirely beneath* the load-line, whereas for very minute forces it lies *entirely above* this line.

With nickel the curves for very minute magnetizing forces are, like those of iron, exactly the same for different values of the force, but instead of lying *above* the load-line lie entirely *below* it. Beyond a certain limit of the force the curves cease to be identical and the maximum decrease of permeability begins to become less and the curve near its origin to bulge more and more towards the load-line, until for a value of force a little over 21 C.G.S. units the permeability begins to increase with small loads and to cut the load-line in one point, which gets shifted further and further away from the origin as the force increases.

Mr. Shelford Bidwell said that Prof. J. J. Thomson, reasoning from the change of length by magnetization, had predicted a Víllari point in cobalt when compressed, and this was verified experimentally. On applying similar reasoning to nickel, he (the speaker) did not expect to find a Villari point, and both Sir W. Thomson and Prof. Ewing had searched in vain for one. In some experiments, not yet completed, he had examined the behaviour of nickel, both loaded and unloaded, when subjected to various magnetizing forces. These show that the metal always contracts when magnetized. For no load the contraction at first increases with the magnetizing force, but attains a maximum. With a moderate load the contraction is less for small forces, but for larger forces becomes equal, and then exceeds the contraction of the unloaded wire. For greater loads the contraction is less than when unloaded for all values of the force.

L. *On* Bertrand's *Idiocyclophanous Prism.*

By Prof. S. P. THOMPSON, *D.Sc.*[*]

(Abstract.)

THIS hitherto undescribed prism is a total-reflexion one, made of calc-spar, which shows to the unaided eye the rings and crosses such as are seen when a slice of spar is examined by convergent light in a polariscope. The space is cut so that the light after the first reflexion passes along the optic axis, and after the second reflexion emerges parallel to the incident light. The rings and brushes are present in pairs, but two pairs may be seen by tilting the prism to one side or the other. This was demonstrated before the Society.

Prof. Thompson also exhibited a similar prism cut from quartz. Owing to the feeble double-refracting of the substance no conspicuous rings could be seen, but when examined by the lantern traces of such rings were visible.

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[*] Read March 21, 1890.

LI. *On the Shape of Movable Coils used in Electrical Measuring-Instruments.* *By* T. MATHER, *Assistant in the Physical Department, Central Institution*[*].

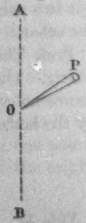
IT is with some diffidence that I venture to bring the subject of this note before the Society, because it concerns such a fundamental point in the construction of measuring-instruments having movable coils, such as d'Arsonval galvanometers, electro-dynamometers, wattmeters, &c., that it is almost certain to have been worked out before. However, as the matter is not touched upon in the ordinary text-books, I bring it forward in the hope that it may serve to recall attention to a subject which, judging from the construction of such instruments as are in common use, has been almost entirely ignored.

In a paper "On Galvanometers," by Prof. Ayrton, Dr. Sumpner, and the writer, read before the Society on January 17th, it was pointed out that the coils of d'Arsonval galvanometers should be narrow and long, and that there should be no internal core. Mr. C. V. Boys had evidently arrived at the same conclusion long before the date referred to, for the coil of his radio-micrometer is a proof of this.

The object of this paper is to determine the best shape of the section of the coil perpendicular to the axis about which it turns.

The subject will be dealt with as concerning coils suspended in uniform fields, but similar reasoning may be applied to other instruments in which movable coils are used.

Fig. 1.



Let the point O (fig. 1) represent in plan the axis about which the coil turns, and assume it is placed in a magnetic field whose direction is perpendicular to A B.

Let P be an element of the section of the coil, then the deflecting moment exerted by unit length measured at right angles to the paper is

$$\mathrm{HC}ar\sin\theta\,; \quad . \quad . \quad . \quad . \quad (1)$$

where H is the strength of field, C the current-density per unit area, a the area of the element, and r its distance from the axis.

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[*] Read March 21, 1890.

The moment of inertia of the element about O will be

$$w a r^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

where w is the mass per unit cube.

Now in ordinary commercial instruments it is important that the period of oscillation should not be inconveniently long, and that the power consumed by the instrument should be as small as possible; then, since for a constant period the controlling moment at unit angle must be proportional to the moment of inertia, the problem resolves itself into finding the shape of the section such that the total deflecting moment for a given total moment of inertia is a maximum.

The ratio of the Deflecting Moment to the Moment of Inertia for the element above considered is

$$\frac{\mathrm{HC}ar \sin\theta}{w a r^2}, \quad i.\ e. \ :: \ \frac{\sin\theta}{r},$$

since H, C, and w may be considered constants.

The ratio $\frac{\sin\theta}{r}$ thus is a measure of the efficacy of the element and its position.

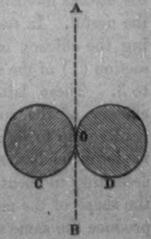
If we now draw a curve whose polar equation is

$$r = x_1 \sin\theta,$$

then, as in Maxwell (vol. ii. p. 332), it may be shown that a given length of wire wound within this space is more efficient than if wound outside it. Hence the curve $r = x_1 \sin\theta$, *i. e.* a circle tangential to A B at O, is the best form of the section of a movable coil, just as in the case of sensitive galvanometers the best shape is given by

$$r^2 = x_1^2 \sin\theta.$$

Fig. 2.



The complete section is given in fig. 2, and consists of two circles touching at O. The circles C and D have section-lines in different directions to indicate that the current passes in opposite directions in the two.

The problem may be treated in another way; for it resolves itself into finding the shape and position of an area having a given moment of inertia about a point in its plane such that the moment of the area about a coplanar line through the point is a maximum.

Taking the point as pole, and the line as the line of reference, the expression

$$\iint r^2 \sin\theta\, dr\, d\theta \text{ is to be a maximum,}$$

whilst

$$\iint r^3\, dr\, d\theta \text{ is constant.}$$

This may be solved by the Calculus of Variations with the result above obtained. By this method Dr. Sumpner (to whom the writer suggested the problem) arrived at the solution some time before the author considered the subject from the first point of view.

In order to illustrate the sort of improvement obtained by winding coils to this shape, a table is appended, the first column of which shows various shapes of section, the second column the value of the ratio moment of area ÷ moment of inertia of area, and the fourth column the deflecting moments of the various shapes all having the unit moment of inertia. From this last column it may be seen that the ordinary Siemens dynamometer type of coil, and those of d'Arsonval's galvanometers and wattmeters, are far from being the most efficacious.

The numbers in column 4 are obtained on the assumption that the coils are long in proportion to their breadth; and when this condition is not fulfilled (as it very seldom is), the numbers would be still further reduced, for the parts of the wire at right angles to the axis of suspension add to the moment of inertia but not much to the deflecting force.

This consideration is of importance where the coils are of sections such as (4), (7), (8), (9); for as the coils are wide, the useless moment of inertia sometimes amounts to one fifth the useful. In such a case as (8) the real numbers representing the efficacy of the coil, as compared with one made to section (1) of the same moment of inertia, would be about 1 to 3. These latter numbers represent approximately the efficacy of the coil of an ordinary Siemens dynamometer reading up to 60 amperes, as compared with that of a coil made to the best shape. It is thus possible to increase the deflecting moment for a given current threefold by changing the shape of the movable coil and arranging the fixed coils to produce the same strength of field as at present used.

Of course the new coil would be heavier than the old one, and would therefore require a stronger suspension which might introduce more vagueness of zero; but as the friction at the mercury-cups is considerable, the necessity for using a stronger suspension would not seriously interfere with the accuracy of the instrument.

It will be noted that in the preceding part of this paper the moment of inertia of the suspended coil has been assumed constant; it is perhaps desirable to give reasons why this assumption is made.

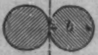
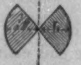
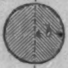
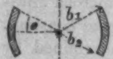
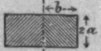
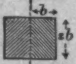
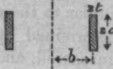
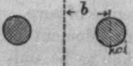
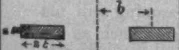
In most instruments having movable coils which are in ordinary use, the current is led into and from the coil either by mercury-cups, or by wires which also serve as the control, or by flexible wires independent of the control.

Taking the first case of mercury-cups, it may be noted that, owing to friction and viscosity, a certain minimum control is necessary to give the requisite definiteness of zero. As time is generally of importance in making commercial measurements, the period of oscillation may be taken as constant. Therefore, as both control and period are determined by circumstances other than those affecting the shape of the coil, the moment of inertia may be taken as constant.

When the current is led into the coil by the torsion-wires, the smallest size of the latter is usually determined by considerations of heating, and their length is limited by questions of portability, compactness, and resistance. This means that the control cannot be diminished below a certain minimum, and hence, by reasoning as in the last paragraph, the moment of inertia of the coil must not exceed a certain value.

The third case, where the leading-in wires are independent of the control, the conditions are somewhat similar to those in the first case; for the size of these wires is determined by considerations of heating by the current to be measured, and they necessarily possess some viscosity or internal friction, which requires a certain control to give definiteness of zero.

In addition to the assumption of constant Moment of Inertia, the field in which the coil swings is taken to be uniform and parallel; and consequently the reasoning will not apply to radial fields which exist in instruments such as the new d'Arsonval milliamperemeter, having a range of 180°.

	Shape of Section.	$\dfrac{\text{Deflecting moment}}{\text{Moment of inertia}}$.	Proportions in particular cases.	Deflecting moment per unit Moment of inertia.	Instrument.
1.		$\frac{4}{3b}$.	………	1·02	
2.		$\frac{4\sin\theta}{3b\theta}$.	$\theta=30°$. $\theta=45°$.	0·91 0·95	
3.		$\frac{8}{3\pi b}$.	………	0·80	
4.		$\frac{4\sin\theta}{3\theta}\,\frac{b_1^3-b_2^3}{b_1^4-b_2^4}$.	$\theta=19°, b_2=\frac{12}{13}b_1$	0·44	Low-resistance d'Arsonval.
5.		$\frac{3b}{2(b^2+a^2)}$	$2a=b$. $4a=b$.	0·97 0·91	
6.		$\frac{3}{4b}$.	………	0·79	
7.		$\frac{3b}{t^2+a^2+3b^2}$.	$a=\frac{1}{5}b,\ t=\frac{1}{15}b$	0·47	Jolin's High-resistance d'Arsonval.
			$a=\frac{1}{4{\cdot}8}b,\ t=\frac{1}{26}b$	0·38	Wattmeter (Ganz and Co.), 60 amperes.
8.		$\frac{b}{b^2+\frac{c^2}{2}}$.	$c=\frac{1}{11}b$.	0·40	Dynamometer (Siemens), 60 amperes.
9.		$\frac{3b}{t^2+a^2+3b^2}$.	$a=\frac{1}{19}b,\ t=\frac{1}{2{\cdot}5}b$	0·53	Dynamometer (Siemens), 500 amperes.
			$a=\frac{1}{22}b,\ t=\frac{1}{2{\cdot}7}b$	0·49	Wattmeter (Siemens), 500 amperes.

In such case it would be advantageous to make the coil of a section similar to that in No. 4 (see table), keeping the radius *b* as small as possible.

The shape given in No. 1 of table cannot be imitated exactly in practice, for insulation-space is required between the two halves, and unless the coil is used in a zero-instrument, parts near the axis might oppose the rest when a considerable deflexion exists. Shapes (3), (5), and (6) are also open to the same objection, but this is easily surmounted by allowing a small space to exist about the axis; in fact some such space is almost necessary to enable the coil to be wound conveniently.

Again, in Siemens dynamometers for fairly large currents it would be difficult to make the coil of the best shape, owing to the space required for the mercury-cups being considerable; but a much closer approximation to this shape than the one often employed could be used.

In ordinary d'Arsonvals, which deflect through a considerable angle, the shape should be modified so that no part of it crosses the line A B (fig. 2) even at the maximum deflexion, for any such part would oppose the rest of the coil. The resulting figure would be lemniscate-shaped. There are some cases, however, such as deflexional dynamometers, where it is advisable to make the coil of such a shape as to cross the line A B when deflected; for then the deflexion would increase less rapidly than the square of the current, and hence the instrument would have a greater useful range for a given length of scale.

Mr. C. V. Boys said he had, when working at his radiomicrometer, arrived at a shape similar to that recommended in the paper. He also noticed a peculiar relation true for all shapes where the length parallel to the axis of rotation is great compared with the breadth. Suppose a coil of any dimensions, then another coil of half the breadth and double the length and cross-section will be dynamically, electrically, and magnetically the same as the original; for the moment of inertia, the electric resistance, and the enclosed magnetic field are equal. The above relation is also true where the breadth is not small if the cross-pieces be thickened near the axis, so as to make their moment of inertia proportional to their length. He inquired whether the author had considered the subject of grading the movable coils; he himself was of opinion that, unlike fixed galvanometer-coils, the wire near the axis should be thicker than that further away.

The President remarked that, in 1881, Prof. Perry and himself exhibited a Wattmeter at the Society of Arts whose movable coil somewhat resembled one of those in the paper, which gave a deflecting moment of 95 per cent. of the maximum. In designing the instrument they had felt that the ordinary method of using a comparatively large swinging coil was not the best, and this led them to the shape adopted.

LII. *Considerations on Permanent Magnetism.*

By M. F. OSMOND[*].

IRON possesses at least two molecular states, α and β. α is soft iron, that is iron which has been annealed and cooled slowly. It changes into β during heating, at a certain critical temperature, and in part preserves this state during cooling, the more so the quicker the cooling and the larger the proportion of carbon, manganese, and tungsten that it contains. Some β iron is also produced during cold hammering. Iron deposited electrically is also of the β variety.

A bar of steel, allowance being made for the carburets and other compounds simply in mixture, can then be looked upon as an intimate mixture of α iron and β iron in relative proportions, which can change from point to point but are determinate at any point. That being so, let us consider β as forming in a steel bar a porous framework, not changing under the influence of currents and of magnets. On the other hand, let us look upon α as composed of particles polarizable under such influences. (The actual mobility of such particles, admitted by Hughes, appears from the sounds and changes of volume which take place during magnetization.) The particles α so polarized will form so many small elementary magnets, which the magnetizing force has displaced from their former normal position of equilibrium, and which tend to take up this position again so soon as the external force ceases to act.

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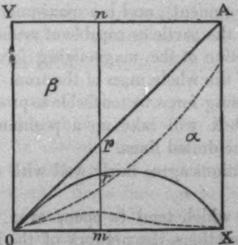
[*] Read April 18, 1890.

But in presence of the rigid network of iron β these polarized particles α are conceived as catching in the pores of the β structure, and immovable in this position, thus resulting in a permanent magnet. The temporary magnetism and the permanent magnetism determined by the action of a certain magnetizing force at a point of a bar will be therefore a function of the relation $\frac{\alpha}{\beta}$ at this point.

As an attempt to represent these ideas graphically, let there be a square, O A X Y, of which the side represents 100 parts. Every line $m\,n$ parallel to the axis of Y meeting in p the diagonal O A represents a steel containing $\frac{mp}{100}$ of iron α, $\frac{np}{100}$ of iron β.

At saturation the temporary magnetism, or rather the sum of the polarized particles, will be exactly equal to the whole of the iron α, *i. e.* to mp. α varying from 0 to $\frac{100}{100}$, the temporary magnetism will be represented by the diagonal OA.



As to the permanent magnetism, in regarding it as the relative number of particles of α which remain polarized after the cessation of the magnetizing force, it will be a fraction of mp, all the greater as $\frac{\alpha}{\beta}$ is smaller for the steel under consideration. In other words, the polarized particles α will be more immovable the closer the network β.

Under the supposition that the quantity mr of the particles α which remain polarized after the interruption of the magnetizing force (that is to say, the permanent magnetism as defined) is proportional to the percentage of iron β, $\frac{np}{100}$, we can write

$$mr = mp \times \mathrm{K}\frac{np}{100},$$

K being a numerical coefficient equal to or less than unity.

Since

$$\begin{aligned} mp &= m\mathrm{O} = x, \\ np &= 100 - mp = 100 - m\mathrm{O}, \\ &= 100 - x, \\ y &= x \times \mathrm{K}\frac{100 - x}{100}, \\ &= \mathrm{K}\left\{x - \frac{x^2}{100}\right\}. \end{aligned}$$

Accordingly the permanent magnetism is represented by a portion of a parabola whose axis is parallel to that of Y. It vanishes for $\alpha = 0$, $\beta = 0$ (as could be foreseen from the properties of the irons α and β, which have served as the grounds of the argument), and is a maximum for $\alpha = \beta$.

The number of the particles capable of remaining polarized after the interruption of the magnetizing force will at most form a quarter of the whole mass of the iron.

If the magnetizing force be too feeble to produce saturation, the lines OA, OrX will take up a position indicated approximately by the dotted lines.

These considerations agree fairly well with many observed facts.

Thus all causes which tend to open, even temporarily or only in certain directions, the network of the iron β (expansion, shocks, vibrations) set at liberty some of the polarized particles of α, and diminish the permanent magnetism.

The greater the proportion of β iron the more difficult becomes the polarization of α, in the diminished pores, and the greater must the magnetizing force be to produce saturation.

When the proportion of carbon or of manganese is so great that the whole of the iron on quenching or even without this takes up the state β, a substance will result which is incapable of taking up magnetism; for example Spiegel-iron containing 25 per cent. of manganese and upwards, and Hadfield's steel. Since hardening takes place more energetically at the surface than in the interior of a bar, the relation $\frac{a}{\beta}$ will always be smaller there. The permanent magnetism remaining after saturation will therefore be stronger at the surface if $a > \beta$, which is the common case. It would be more feeble if $\beta > a$.

Magnets formed of laminæ are in general more powerful than those of the same volume in a single piece, because the laminæ are more hardened. But the contrary should be the case with metals in too hard a condition.

It is difficult after a bar has been magnetized to saturation in one direction to bring it to a moment of the same value in an opposite direction, because certain particles of the a metal being held immovably cannot be displaced, &c.

The iron β can also be looked upon as a medium whose viscosity increases as its percentage in the mixture is greater.

We can compare these ideas with the experiment of Hughes, who has obtained permanent magnets by mixing iron filings and wax and melting the latter, which in setting held fixed the polarized portions of iron.

Mr. Swinburne asked if the theory would account for the increase of induction which occurs when the circuit of a permanent magnet is closed; most theories founded on the orientation of particles by the magnetizing force seemed defective in this respect. Some time ago he had suggested that the permeability of iron should be tested by first magnetizing it one way, and then at right angles to the first direction; recently he had been informed that no increase of permeability was observed when the experiment was performed.

Prof. Perry said he had subjected iron to magnetization in one direction, and found the permeability for small forces in a direction at right angles much smaller than he had anticipated; the first magnetizing force was kept constant when the small perpendicular one was applied.

Mr. Swinburne thought that for such small perpendicular forces the permeability should be nearly infinite. He also said there seemed a sort of angular hysteresis in iron; for if a loose-running armature was turned slowly round by hand, it would come back two or three degrees when left free.

The President remarked that, as far as he could see, M. Osmond's theory does not account for the great influence which a small percentage of tungsten has on the magnetic property of steel, and all theories which failed in this particular must necessarily be imperfect.

LIII. Prof. RÜCKER described the results of *some Recent Magnetic Work* undertaken by himself and Prof. Thorpe in connexion with their Magnetic Survey of the United Kingdom*.

IN a paper read before the Royal Society last year they have shown that the United Kingdom can be divided into seven or eight distinct districts, in each of which a source of disturbance seems to exist. The probable cause of these disturbances the authors believe to be the presence of "magnetic rocks." If this be true, an intimate connexion should exist between the magnetic elements of a district and its geological structure. If a magnetic mass be supposed to exist below the surface, then one would expect the vertical force to be greatest above that mass, whilst the horizontal disturbances would tend towards the mass, and would differ in direction on its opposite sides.

With a view to testing more fully than heretofore the magnetic constitution of the districts above referred to, the work (the results of which Prof. Rücker now described) was undertaken.

Two test districts were selected in which numerous observations were taken; one of these is situated on the west coast of Scotland, and the other embraces parts of Lincolnshire and Yorkshire. The first of these districts was chosen for two reasons:—First, because it is very disturbed, and, owing to the vast masses of basalt, would be a specially difficult one; and secondly, because it is near the borders of their survey, and is thus particularly open to suspicion that the calculated values of the elements may be inaccurate.

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* Read April 18, 1890.

By the aid of magnetic maps, Prof. Rücker showed that a centre of disturbance exists to the west of Iona and south of Barra. Both the vertical force and the horizontal disturbances indicate the same position, and hence favour the hypothesis.

In the Lincolnshire and Yorkshire district, extending from the Wash to Appleby, there is a region in which the horizontal disturbances along the East side tend towards the West, whilst along the West side they are, on the whole, directed easterly. This points to a ridge of magnetic material along the middle of the region, and from numerous observations the approximate position of the ridge-line has been determined over a length of 150 miles. In Linconshire the ridge follows closely the line of Wolds, whilst at the Kettlewell end it is directed towards the Cumberland Lake-district. Places of maximum vertical force occur near Market Weighton and Harrogate.

The observers were directed to Market Weighton by Prof. Judd as being situated where the Liassic strata thins out. Harrogate stands on the summit of an anticlinal. The fact that such places of maximum vertical force exist where, according to hypothesis, they would be expected, tends to confirm the supposition, and thus to demonstrate that a very close connexion exists between the magnetic elements and the geological structure of a country. This is further confirmed by the observation that in Lincolnshire the line of disturbance follows closely the line of the Wolds, and that from Market Weighton it passes across the plain of York to the Millstone Grit and Limestone, which are the oldest rocks in the neighbourhood.

Mr. Whipple said he had listened with very great pleasure to the interesting details and results given by Prof. Rücker. Never, to his knowledge, had a magnetic survey been conducted with such minuteness and accuracy. The older surveys related chiefly to declination, whilst the determinations of intensity were made in a very primitive way. He looked forward with interest for other important results, which he felt sure would follow from the admirable survey of Professors Rücker and Thorpe.

LIV. *On the Distribution of Flow in a Strained Elastic Solid.*

By CHARLES A. CARUS-WILSON, *B.A., A.M.I.C.E., Demonstrator in the Mechanical Laboratory at the Royal Indian Engineering College, Coopers Hill*[*].

IF a metal bar of uniform section throughout its length be subjected to uniform longitudinal stress, the elements of the bar will become distorted; if the strain should exceed the limit of elasticity the distortion will be partly permanent.

In consequence of the straining of the elements, every element will experience a displacement relative to three fixed axes in the bar; say ox parallel to the length of the bar, oy parallel to one side, and oz parallel to a second side, both oz and oy being at right angles to ox: such displacement is generally spoken of as "flow."

It is clear that if the metal be homogeneous, the stress uniform, and the section at right angles to the axis everywhere the same, an element will experience a displacement at right angles to the planes xoz, yoz proportional to its distance from these planes; since such displacement depends on the straining of the elements between it and these planes. Hence a bar with its sides originally parallel to ox will remain so after a certain amount of flow has taken place, and lines drawn on the sides respectively at right angles to and parallel to ox will remain so, since the displacement parallel to ox will be proportional to the distance from the plane yoz.

The question I propose to discuss is how to determine the distribution of flow when the section at right angles to ox varies in any way as we pass along ox.

I will for simplicity consider a plate in which the thickness parallel to oy is considerable compared with the thickness parallel to oz, and I will consider the displacements in the plane xoy.

We may take a plate in which there is a gradual reduction of section, in which there is what is commonly called a "shoulder," as in fig. 2.

It is required to determine the displacements in the plane of the paper—the plane xoy: in other words, supposing the

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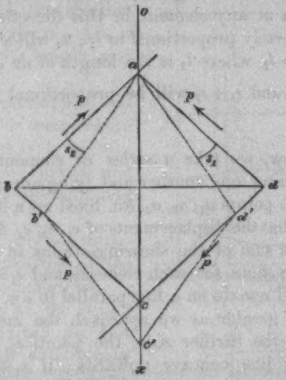
[*] Read May 2, 1890.

plate covered with a series of lines parallel to *o x* and to *o y* respectively, it is required to determine the curvature of these lines due to flow.

The distortion of the elements is caused by the shearing-strain induced in the bar when subjected to longitudinal stress. Since this shearing-strain is a maximum at 45° to the axis *o x* of the bar, it will be convenient to consider the bar divided into a number of small elements with sides making angles of 45° to the axis *o x*. Every such element as *a b c d* (fig. 1) will be subject to a shearing-strain of amount $\frac{p}{n}$; *i. e.* the angle *b a d* will be diminished by an amount $\frac{p}{n}$, where p is the intensity of the shearing-strain on the element, and n is the coefficient of rigidity of the metal. The effect of this shearing-strain on the element may be described as a rotation of *a d* through an angle s_1, and of *a b* through an angle s_2, where $s_1 + s_2 = \frac{p}{n}$; s_1 and s_2 may be equal, when the displacement of *c* will be wholly parallel to *o x*; or they may be unequal, when *c* will have a displacement parallel to *o x* and a resolved displacement parallel to *o y*, which will be +ve or −ve

Fig. 1.



according as s_1 is less than or greater than s_2, and which will be a maximum if either s_1 or $s_2=0$.

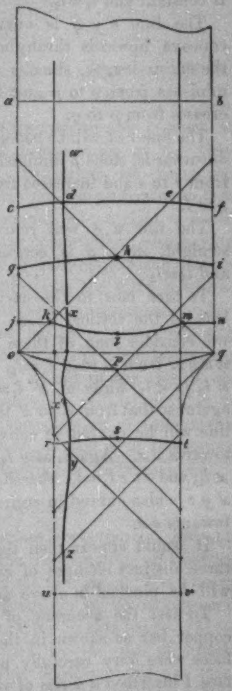
The value of s_1+s_2, and the ratio of s_1 to s_2 for any element, appears to be determined by the following considerations. If a side ab of the element be produced in both directions so as to cut the sides of the bar, there is seen to be a tendency of that part of the bar below this line to slide over that part which is above. Assuming the resulting shearing-strain to be uniform over the length l_1 of this oblique section, the shearing-strain at any element in this direction, *i. e.* s_1, will clearly be inversely proportional to l_1; s_2 will also be inversely proportional to l_2, where l_2 is the length of an oblique section parallel to ad, and s_1+s_2 will be proportional to $\frac{l_1+l_2}{l_1l_2}$—say to $\frac{1}{\rho}$.

Suppose, now, we take a series of elements, $a_1b_1c_1d_1$—$a_2b_2c_2d_2$ touching one another and lying across the bar. If we imagine the points a_1, a_2, a_3, &c. fixed on a line parallel to oy, it is clear that the displacements of c_1, c_2, c_3, &c. will be proportional to the sum of the shearing-strains in each element. If s_1+s_2 is the same for each element, and $s_1=s_2$, the points c_1, c_2, c_3, &c. will remain on a line parallel to oy. If, however, s_1+s_2 becomes greater as we approach the middle, and less again towards the further side, the points c_1, c_2, c_3, &c. will be found on a line concave upwards; if s_1+s_2 diminishes towards the centre, these points will lie on a line concave downwards. If the strains should exceed the limit of elasticity, the line through c_1, c_2, c_3, &c. will be permanently curved.

If the points a_1, a_2, a_3, . . . , instead of being in one straight line have unequal downward displacements, the form of the curves obtained as above may be modified.

If, now, we consider a series of consecutive elements, $a_1b_1c_1d_1$—$a_1b_2c_2d_2$, lying in a line parallel to ox, if $s_1=s_2$ for each element the points c_1, c_2, c_3, &c. will continue to lie on a line parallel to ox, since the displacement of each of these points, due to the distortion of the corresponding element, is parallel to ox. But if as we descend from the first element we find s_1 becoming greater than s_2, the points c_2, c_3, &c. will have a displacement to the left which increases as we descend, so the line passing through c_1, c_2, c_3, &c. will be bent; and if the shearing-strains become equal again on the lowest element, this line will be concave to the right.

Fig. 2.



It thus appears that any horizontal line will be curved concave downwards if the sum of $s_1 + s_2$ is greater at the end of the line than in the centre, and *vice versâ*, but that the line will remain straight if $s_1 + s_2$ is constant along its length, and $s_1 = s_2$.

Also that a vertical line will remain so if s_1 is everywhere equal to s_2; but if s_1 is $> s_2$, the line will be curved and concave towards the right, and *vice versâ*.

We can now proceed to determine the curvature of the lines in the case before us.

Commencing at the upper extremity of the bar, fig. 2, a line such as *a b* will remain straight, because ρ is everywhere constant, and $l_1 = l_2$. The line *c d e f* will be curved concave downwards, since from *c* to *d* and from *e* to *f*, ρ is less than it is from *d* to *e* (where it is constant and $l_1 = l_2$); *d e* will therefore remain straight, and *c d* and *e f* curved as shown. The line *g h i* will be concave downwards and curved along its entire length, since ρ gets less as we move away from *h* on both sides.

The line *j k l m n* will remain straight from *j* to *k*, since ρ is there constant and $l_1 = l_2$; from *k* to *l*, ρ becomes less and greater again from l to m, $k\ l\ m$ is therefore concave upwards; this line is straight from m to n, since ρ is constant and $l_1=l_2$.

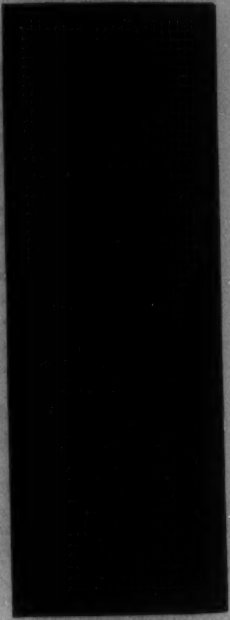
The line $o\ p\ q$ is curved concave upwards throughout the entire length, since ρ diminishes from o to p and increases from p to q.

The line $r\ s\ t$ will be concave downwards, since ρ diminishes from r to s and increases from s to t.

The line $u\ v$ will remain straight, since ρ is constant and $l_1=l_2$

To turn now to the curvature of the vertical lines. If we consider one of these as $w\ x\ y\ z$, from w to d $l_1=l_2$, at x l_2 is $<l_1$, while at x' $l_1=l_2$ again, so that from d to x' this line will be curved in convex towards $o\ x$. Again, at y l_2 is $<l_1$, and at z $l_1=l_2$, therefore $x'\ y\ z$ is also curved in convex towards $o\ x$.

Fig. 3.



It would appear then that the horizontal lines will have three distinct changes of curvature, while the vertical lines will be pinched in above and below the shoulder.

To test the accuracy of these conclusions, I prepared a copper bar as shown in the following way. The sides and faces were very carefully planed and polished, and on one face I inscribed a series of vertical and horizontal lines in a lathe. I then strained the bar so as to produce a considerable permanent set. An impression was then taken from the bar, which is reproduced in fig. 3[*].

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[*] The diagonal lines in fig. 3 were ruled on the impression to serve as lines of reference.

By placing the eye very nearly in the plane of the paper and looking along the lines, the curvatures can be seen to follow very closely those sketched on fig. 2. I would specially draw attention to the partial curvature of such lines as *c d e f* and *j k l m n*.

LV. *Galvanometers.* *By* Prof. W. E. AYRTON, *F.R.S.*, T. MATHER, *and* W. E. SUMPNER, *D.Sc.*[*]

IN order that a number of students may be able to work at the same time in the Physical Laboratories of the City and Guilds of London Central Institution we have endeavoured, as far as possible, to arrange the apparatus so that each of the many experiments should be complete in itself. This has led to the necessity of our constructing, and having constructed for us, a large number of galvanometers of various types, and from the specimens that are on the table it will be seen that several of the galvanometers contain points of novelty. A long series of comparative tests has been carried out with all the more important types that are in our laboratories, and we have therefore thought that a record of the results obtained by the students, and by ourselves, will be of value in affording data to guide others in the selection of instruments most suitable for the objects in view, as well as in suggesting future improvements in the manufacture of galvanometers.

I. *Astatic or Non-Astatic.*

One of the first questions that arises is whether it is desirable to employ the astatic type in the construction of sensitive galvanometers. Prof. A. Gray, in his treatise on 'The Theory and Practice of Absolute Measurements in Electricity and Magnetism,' maintains that it is not, for he says (page 311):—"Sensibility is sometimes obtained by the use of astatic galvanometers, but these are rarely necessary and are more troublesome to use than ordinary non-astatic instruments."

This opinion carries special weight as it may be very probably taken as expressing Sir William Thomson's view on this subject, seeing that the usual method adopted by Sir

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[*] Read January 17, 1890.

William for measuring small currents is to employ a galvanometer with a single set of magnetic needles, and to weaken the magnetic controlling field as far as requisite by means of adjustable magnets. Theoretically there is no limit to the sensibility obtainable in this way; but with any strength of the resultant controlling field due to the earth and the adjustable magnets an astatic combination of magnetic needles must give a more sensitive arrangement than a non-astatic one. For let F be the resultant strength of this controlling field, let M be the magnetic moment of a needle within the coil whose magnetic constant is G, and let C_1 be the current flowing, then, if α be the deflexion, we have for small deflexions

$$\alpha = \frac{C_1 G}{F}.$$

Next, let a needle of moment m be attached to the suspended system outside the coil, so as to form an approximately astatic system with the needle inside, then, even if we neglect the action of the coil on the needle outside, an action which of course assists the action of the coil on a needle inside, we have, if C_2 be the current now flowing,

$$\alpha = \frac{C_2 GM.}{F(M-m)}.$$

Hence, if the deflexions be the same in the two cases,

$$C_2 = C_1 \frac{M-m}{M}.$$

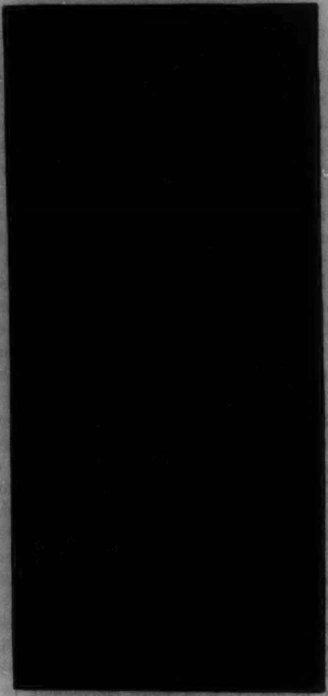
Now M may be made large, and $M-m$ small, hence C_2 may be a small fraction of C_1. Consequently for any adjustment of the controlling magnet, the current required to produce a given deflexion can be much diminished by using an astatic combination instead of a non-astatic one.

But, perhaps, the most important advantage of an astatic over a non-astatic instrument is its much greater freedom from outside magnetic disturbance. In order that C_1, the current with the non-astatic galvanometer, may be small for a given value of the deflexion α, the resultant controlling field F must be small; whereas with the astatic combination there is no necessity to make F small in order that C_2 may be small, since $\frac{M}{M-m}$ may be made large. Now the disturbance that an extraneous magnetic body can bring about depends on the strength of the magnetic field it sets up compared with that of the field F produced by the earth and the adjusting magnet combined. Hence the moving about of a magnetic body in the neighbourhood of the galvanometer will produce more disturbance on a non-astatic galvanometer than on an astatic galvanometer of the same sensibility. The preceding are very important considerations; hence, so far from agreeing with Prof. Gray that it is more troublesome to work with astatic galvanometers than with non-astatic ones, the preceding considerations, which we find fully borne out by experience, have led us to regularly employ the astatic principle with sensitive galvanometers.

And generally we may conclude that when it is desired to utilize a magnetic control, for example, with electrometers, &c., it is better, when a small controlling moment is required, to obtain this by the use of a weak magnet on the suspended system than by attempting to simply weaken the controlling field. Indeed, we may mention that even in the case of electrometers, where there is no question of the action of currents on magnets, and where (when a magnetic control has been employed in place of a bifilar or torsional control) it has been usual to attach a single little magnet to the aluminium electrometer-needle, to be acted on by a stationary outside magnet, we have found it more convenient to attach an *astatic* combination of magnets to the moving needle, and to direct the system by a fairly strong permanent magnet, which acts of course differentially on the suspended astatic system of magnets, since with such an arrangement great freedom from outside magnetic disturbance is secured, combined with the power of employing a wide range of sensibility.

It is worth noticing that in galvanometers, where the distance of the controlling magnet from the needles is varied by simply raising the magnet M, as in the case of the galvanometer seen in fig. 1, it is more convenient to place the galvanometer so that the plane of the coils is in the magnetic meridian of the laboratory when the controlling magnet is removed; or, in other words, to place the galvanometer so that the spot of light comes to the zero of the scale for no current passing through the instrument and with no controlling magnet. For in that case the sensibility of the instrument can be subsequently varied by simply raising or lowering the controlling magnet *parallel to itself*; whereas, if the galvanometer be fixed in any other position, it is necessary, when raising or lowering the controlling magnet, to give it a care-

Fig. 1.

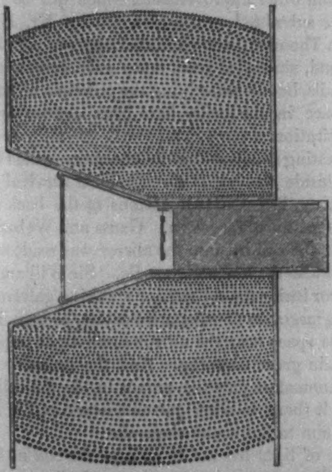


ful screwing motion to prevent the spot of light going off the scale.

II. *Position of Mirror.*

According to the ordinary method of constructing a Thomson's reflecting-galvanometer, a tubular space at the centre of the coil is left unwound, and the mirror, with the magnets at the back of it, hung at the centre of this unwound space (see fig. 2, which illustrates a well-known form of this type of galvanometer). To allow the ray of light to pass out of the coil after reflexion at the mirror, when the mirror is deflected, the end of the tubular space has to be made trumpet-shaped, which leads to still more space in the neighbourhood of the needle being left unwound. And when the galvanometer is an astatic one, a somewhat similar tubular space is left unwound on each side of the lower coil to allow room for the diamond-shaped aluminium vane to turn.

Fig. 2.



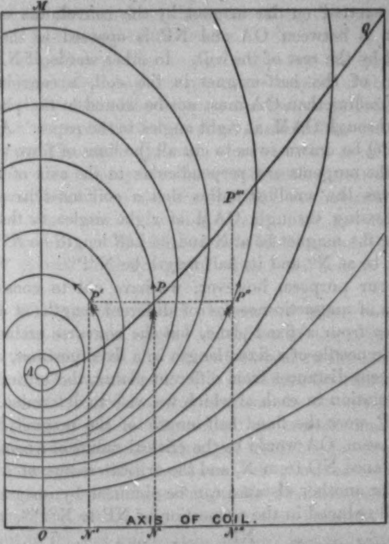
In this way the most valuable part of each coil, viz. that close to the magnetic needles, is left unwound. This disadvantageous mode of construction so impressed itself on Mr. Mudford, while a student at the City Guilds' Institute, that he suggested that the needles alone should be placed within the coil, and that the mirror (which is, of course, employed to reflect a ray of light, and not, as its position *inside* the coil would seem to infer, to be acted on electromagnetically) should be placed outside the coil, or between the upper and lower sets of coils when two pair of coils are employed. A specimen of a galvanometer constructed in this way and containing certain other improvements, which will be referred to later on, was submitted some years ago by Mr. Mudford to Sir William Thomson, who expressed approval of the devices employed, and, since that time, many galvanometers differing in other details, but all embodying the principle of not wasting valuable space in the coils, have been constructed for the Central Institution.

It is interesting to notice that the common method of placing the mirror inside the coil is an irrational survival of an old custom kept up, like the two buttons at the back of a coat, when its use is almost forgotten. Gauss and Weber put their mirror inside the coil because the mirror was made of polished steel and was also the magnetic needle. Sir William Thomson put the mirror inside the coil in his "speaking galvanometers," for receiving messages on submarine cables, because he desired to render the space near the mirror as air-tight as possible in order to obtain great damping. And the ordinary maker of astatic galvanometers puts the mirror inside the coil, because he has seen it there in other galvanometers, and it has never occurred to him to put it anywhere else.

If the ray of light has not to go into the coil no hole need be left and the coil may be wound practically to the centre. It is necessary, however, that the space left unwound inside the coil should be somewhat larger than is required simply to allow the needle to turn through a small angle, because if the windings were carried to the axis of the coil in the immediate neighbourhood of the needle the effect on the latter of the current passing through the innermost coils would be opposed to that of the rest of the coil. To find the shape and volume of the least space that should be left unwound we proceeded as follows:—

Let OA (fig. 3) be the radius of a convolution of wire whose plane is perpendicular to the paper, the section of the convolution being represented by the small circle at A, then, from

Fig. 3.



Maxwell's 'Elect. and Mag.' vol. ii. plate xviii., the lines of force due to a current passing round this convolution are as indicated. Therefore, if a magnet whose half length is NP be placed at a distance ON from the plane of the convolution such that NP is a tangent to the line of force at P, no torque will be exerted on the magnet by a current passing round the convolution, since the direction of the force at P is along the magnet NP. But if the magnet be placed nearer to the convolution, as at N′P′, or farther from it, as at N″P″, there will be a torque exerted on it due to this convolution of radius OA; the two torques, however, will be in opposite directions.

Now as the radii of the greater number of convolutions contained in the coil are greater than the half length of the magnet, it is clear that the torque exerted by the convolution OA when the magnet is to the right of NP is in the same direction as that exerted by the coil as a whole, and that the torque exerted on the magnet by this convolution when the magnet is between OA and NP is opposed to the torque exerted by the rest of the coil. In other words, if NP be the position of the half-magnet in the coil, a convolution of smaller radius than OA must not be wound in the plane that passes through OAM at right angles to the paper. And if a line APQ be drawn so as to cut all the lines of force at points where the tangents are perpendicular to the axis of the coil, OA gives the smallest radius that a coil must have in the plane passing through OAM at right angles to the paper, whether the magnet be at N and its half length be NP, or the magnet be at N″ and its half length be N″P‴.

For our purpose, however, we have not to consider the problem of magnetic needles of different lengths at different distances from a fixed plane, but the converse problem of a magnetic needle of a fixed length in a fixed position, which is at different distances from different planes, the critical radius of convolution in each of which we wish to determine.

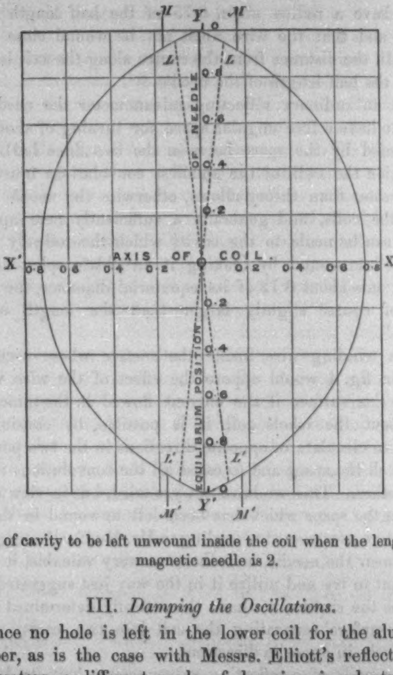
If NP were the fixed half length of the magnet, then, as already seen, OA would be the critical radius of a convolution at a distance NO from N, and the critical radius of a convolution for another distance can be obtained by imagining the figure 3 reduced in the proportion of NP to N″P‴, in which case a critical radius $\frac{NP}{N''P'''} \times OA$ will be obtained at a distance $\frac{NP}{N''P'''} ON''$ from N. Or, lastly, if we take OA as the half length and position of our magnetic needle, the critical radius of convolution at every distance from O will be obtained by taking every such ordinate as N″P‴ and multiplying OA by the ratio of OA to N″P‴, and the distance from O for which this is the critical radius is equal to $\frac{OA}{N''P'''} \times ON''$. A curve giving the locus of such critical radii of convolution is shown in fig. 4 for a needle of length 2, shown therefore in the figure about twelve times its full size, and we learn from it that at a distance from the centre of the coil equal, say, to 0·4 of the half length of the needle, the smallest convolution should have a radius about 0·75 of the half length of the needle, and that the wire must not be wound close to the axis until the distance from the centre along the axis is about 0·72 of the half length of the needle.

With an ordinary reflecting-galvanometer the needle requires to have a free angular space for turning of about 15°, represented by the space between the two lines LOL′. In this region the radii of the smallest convolutions must be a little greater than theory allows, otherwise the needle would touch the coils, and generally a sufficiently near approximation can be made to the cavity which theoretically ought to be left unwound by making it an oblate spheroid with a polar axis about 0·72 of its equatorial diameter, the latter being of course slightly larger than the length of the needle.

Since winding wire inside the surface whose section is given in fig. 4 would oppose the effect of the wire wound outside this surface if the current flowed in the same sense throughout the whole coil, it is possible, by causing the current to circulate in opposite directions in the two portions, to wind all the space and to cause all the convolutions to help one another. This we have not yet tried, but in view of the fact that the space which has been left unwound in the galvanometers constructed according to Mr. Mudford's suggestion is very near the needle, and therefore very valuable, it seems important to try and utilize it in the way just suggested. In that case the surface we have theoretically determined would be the surface separating the coil into two parts, wound respectively in opposite directions.

As the coils of a reflecting-galvanometer have to be supported by a central framework, there is necessarily a certain distance left between the coils when they are in position. This space we find when drawn to scale is that contained between the two vertical lines MM′, and which therefore allows more than sufficient room for the needle to turn. Hence if the plan of opposite winding that we have suggested be tried, the face of the coils may be made quite plane and no portion whatever of the coils left unwound, even to allow the needle space to move.

Fig. 4.

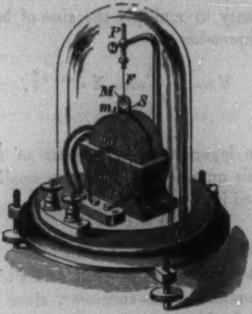


Shape of cavity to be left unwound inside the coil when the length of the magnetic needle is 2.

III. *Damping the Oscillations.*

Since no hole is left in the lower coil for the aluminium damper, as is the case with Messrs. Elliott's reflecting-galvanometers, a different mode of damping is adopted. The plan we find best is to attach the mirror M (fig. 5) to the middle of a long vertical strip of mica, S, across the upper and lower ends and the middle of which the magnets m_1, m_3, and m_2 are fixed. Such an arrangement is also seen in position in the galvanometer shown in figure 6 later on. With this arrangement, which requires very little space, we find that when the controlling magnet is adjusted so that the periodic time of vibration is 7 seconds or more, the spot of light does not visibly pass through the zero

Fig. 5.



or the instrument is dead-beat, and even when the period is much shorter than this, the spot of light does not pass through the zero more than twice*. With galvanometers in which less damping is desired, a portion of the strip of mica is cut away, so as to make it narrower, and in extreme cases, when for example no damping is required, as in the case of a ballistic galvanometer, the mica strip is replaced by a vertical piece of wire.

With a ballistic galvanometer, especially when intended for teaching-purposes, it is desirable to have some easy means of adjusting the amount of damping. In the case of the galvanometer used by our students for experimenting on the variation produced in the swing of the needle of a ballistic galvanometer by variations in the amount of damping, this variable damping is effected by enclosing the mirror in a cell whose glass ends can be simultaneously made to approach or recede. This motion of the ends of the cell is effected by turning a milled head outside the instrument which works a right-and-left-handed screw inside.

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* Quickness in the needle coming to rest, which is produced by using a powerful controlling field, must not, of course, be confounded with considerable damping, since, other things being the same, the decrement is the greater the weaker the controlling field and the slower the motion of the needle.

In order to correct the throw of a ballistic galvanometer for the damping of the oscillations of the needle, it is well-known that it is necessary to multiply the sine of half the angle of throw by the expression

$$V = \frac{1}{\sqrt{1+\frac{\lambda^2}{\pi^2}}} \Sigma^{\frac{\lambda}{\pi}\tan^{-1}\frac{\pi}{\lambda}},$$

where λ is the logarithmic decrement of the oscillations. The value of this quantity when λ is so small that its square may be neglected is

$$V = 1 + \tfrac{1}{2}\lambda.$$

It seemed, however, desirable to work out the value of V more fully and also to express it in terms of the numerical ratio of each amplitude to its successor, since in this way the need of finding the Napierian logarithm is avoided. This we did at first by direct calculation, but while forming a table of values connecting V and the decrement we observed that the matter could be treated much more simply as follows :—

By taking logarithms of the complete expression for V we have

$$\log V = -\tfrac{1}{2}\log(1+x^2) + x\tan^{-1}\frac{1}{x},$$

where

$$x = \frac{\lambda}{\pi}.$$

Differentiate both sides of this equation once, twice, and thrice with respect to x and substitute for x the value 0. We then obtain successively,

$$\frac{dV}{dx} = \frac{\pi}{2}\,; \qquad \frac{d^2V}{dx^2} = \frac{\pi^2}{4} - 3\,; \qquad \frac{d^3V}{dx^3} = \frac{1}{8}\pi^3 - \frac{9}{2}\pi.$$

By Maclaurin's theorem we then have

$$V = 1 + \frac{\pi}{2}x + \left(\frac{\pi^2}{4} - 3\right)\frac{x^2}{2} + \left(\frac{1}{8}\pi^3 - \frac{9}{2}\pi\right)\frac{x^3}{6} + \&\text{c.};$$

or substituting for x, and working out numerically, we obtain

$$V = 1 + 0{\cdot}5\lambda - 0{\cdot}027\lambda^2 - 0{\cdot}054\lambda^3,$$

which shows that until λ approaches unity the approximate value for V generally used is sufficiently accurate. Now if the ratio of the amplitude of any swing to that of its successor is $(1+y)$, we have

$$\lambda=\log(1+y)=y-\frac{y^2}{2}+\frac{y^3}{3}-\&c.,$$
$$\lambda^2=y^2-y^3,$$
$$\lambda^3=y^3.$$

Neglecting powers higher than the third, we obtain by substitution

$$V=1+0\cdot 5y-0\cdot 277\,y^2+0\cdot 130y^3,$$

from which we can at once calculate the value of the complete correcting factor when we have observed the decrement.

It is usual to determine the sensibility of a ballistic galvanometer either by the employment of an earth-inductor, or by discharging through the galvanometer a condenser charged to a known P.D. The first method, however, necessitates an *exact* knowledge of the horizontal or vertical component of the magnetic intensity at the spot, while the second requires an *exact* knowledge of the value of the capacity of the condenser and of the P.D. employed. But now that it is possible to obtain an ammeter calibrated with a high degree of accuracy, the simplest method of determining the sensibility of a ballistic galvanometer is to first calibrate it absolutely as a galvanometer for measuring currents by direct comparison with the ammeter. Its absolute calibration as a ballistic galvanometer can then be determined from the fact that

The swing per micro-coulomb in scale-divisions

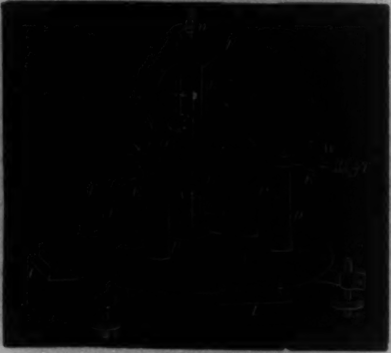
$$=\text{the deflexion per micro-ampere}\times\frac{2\pi}{T},$$

where T is the periodic time of oscillation of the needle in seconds.

IV. *Supporting the Coils.*

In olden days Messrs. Elliott supported the coils in their galvanometers by simply employing three long screws to squeeze each coil between a brass plate and the framework of the instrument to which the plate was screwed. The disadvantage, however, of such an arrangement was made painfully clear in the use of a galvanometer constructed by this firm for one of the authors in 1868. For on transporting this galvanometer across India, soon after it was made, the brass screws expanded more than the coils, and the coils consequently slipped down, shearing away the needles, the mirror, and the aluminium vane. It was therefore suggested to Messrs. Elliott that the coils should be supported in boxes B, preferably hinged, as shown in fig. 6, so that when the coil-boxes are opened, as in this figure, the suspended system S could be got at and even removed without detaching the connecting wires from the coils. The galvanometer shown in fig. 6 is, when in use, covered up by the brass case supporting the controlling magnet, illustrated in fig. 1.

Fig. 6.



Hinged boxes containing the coils of a reflecting-galvanometer appear to have been employed even earlier than this by Mr. Pepper, at the Polytechnic, but that form of construction was, apparently, in 1868 unknown to, or at any rate unused by, instrument makers.

In the case of galvanometers intended to be highly insulated, these hinged boxes may with great advantage be made of ebonite, and to prevent the boxes becoming electrified and acting on the suspended needle electrostatically, one terminal of the galvanometer should be joined to the outside brass case.

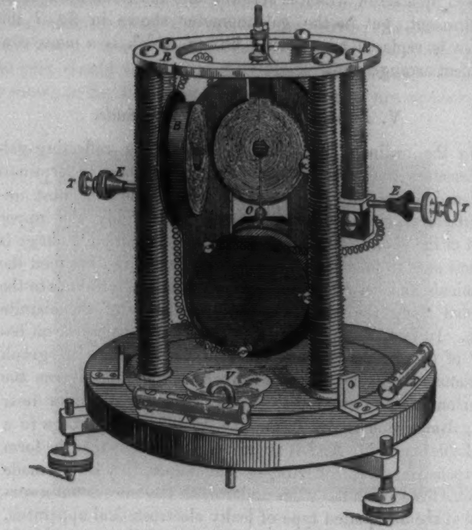
In the galvanometer shown in fig. 6, the boxes are kept closed by a screw which is screwed into the framework of the instrument, but in the galvanometer shown in fig. 7 this screw is replaced by a spring button, which is a more convenient arrangement.

V. *Insulation of Coils and Terminals.*

In the ordinary method of constructing reflecting-galvanometers the wires are brought under the base to terminals fastened on to it. But such an arrangement is most unsatisfactory, since, no matter how thoroughly the upper part of the ebonite base may be cleaned and dried, leakage is almost sure to take place between the two wires, between the terminals, and between both the wires and the terminals to the ground along the inaccessible under surface of the ebonite base. In fact, when one of the authors was engaged, on behalf of the Indian Government, in 1872 testing telegraph insulators at Messrs. Siemens's works at Charlton, it was the common practice every morning to light a gas-burner near the galvanometer to dry it, and to diminish the leakage to a workable amount. And it appears to the authors that the form of expensive astatic reflecting-galvanometer, that is still made by some firms, with the wires underneath the base, is but a survival of the antiquated type of leaky electrostatical apparatus, to use which required that the whole air of the room should be first dried. Fig. 6 shows the obvious way of overcoming this difficulty, and which consists in supporting the framework and the coils of the apparatus as well as the terminals on paraffined corrugated ebonite pillars *p*, which are maintained clean and dry by their being kept entirely inside the brass cover of the instrument Q (fig. 1). The terminals T at the ends of the wires W project through holes H in the brass case, these holes being kept closed by tightly-fitting ebonite collars C, fig. 1, E, fig. 6, when the galvanometer is not in use, or when it is being employed for some test where a small amount of leakage is not important.

Still higher insulation is obtained with the galvanometer shown in fig. 7, since in this instrument the coils are supported from two corrugated ebonite rods which hang from

Fig. 7.



Very high resistance galvanometer with brass case removed.

a brass ring R, carried on the top of three corrugated ebonite pillars fixed to the slate base-plate. This instrument, the four coils of which have a joint resistance of nearly 400,000 ohms, was constructed by Messrs. Nalder Bros.; but the device, by means of which the shortest path along which surface-leakage can take place from the coils, or from the terminals, to the base of the instrument is between 30 and 40 inches of ebonite artificially dried by sulphuric acid, is due to Messrs. Eidsforth and Mudford. To prevent any possible overflowing of the sulphuric-acid vessel, a syphon passes through the base on the Tantalus cup principle, and which empties the liquid when it rises to the level of the bend of the syphon.

VI. *Proportionality of Deflexion and Current.*

Some authorities are of opinion that it is of comparatively little consequence what is the law connecting deflexion and current, since for very accurate work any galvanometer must be carefully calibrated experimentally, and the exact law connecting deflexion and current thus ascertained. But, on the contrary, although the necessity of calibration for accurate work is perfectly true, experience has shown us that for ordinary practical work much time is saved if the readings on the scale are approximately, even if not quite accurately, proportional to the currents producing them. This latter result can, of course, be obtained either by fitting the lengths of the divisions on the scale to the peculiarities of the galvanometer, or by starting with a uniformly divided scale and constructing the galvanometer in such a way that equal additions to the current produce equal angular deflexions.

In certain cases, when, for example, the galvanometer is mainly employed for measuring currents all having about one definite value, as for example in the case of a voltmeter used on an electric-lighting circuit, it is obviously desirable to have the scale widely extended at the part where it is most used, and to obtain this result it is better to have the divisions crowded together elsewhere. But, since the power of accurately subdividing the spaces on a scale by eye is much increased if all the spaces be of equal length, it is preferable, in the case of galvanometers for general use, and when all parts of the scale are equally valuable, to judiciously construct the galvanometer in such a way that the whole scale may be divided into equal distances which are directly proportional to equal increments of current, than to give the coils and needle a shape arrived at in a haphazard fashion, and then attempt to experimentally subdivide the scale to suit the vagaries of the galvanometer.

In a former paper communicated by Prof. Perry and one of the authors to this Society, it was pointed out that while a galvanometer may be a "proportional" instrument if the zero for no current be that corresponding with the needles being in a symmetrical position to the coils, the same instrument may be very far from proportional if the zero for no current be taken at one end of the scale and the spot of light deflected right across the scale for increasing currents, since in this latter case a variation in the law occurs as the plane of the needles passes through the symmetrical position. This want of proportionality is exhibited by the following figures :—

Ordinary Reflecting-Galvanometer.

Deflexion from one end of the Scale.	Relative Strength of Current.	Error.	Percentage Error.
0	0	0	
60	62·0	+2·0	3·4
120	121·0	+1·0	0·8
180	181·5	+1·5	0·9
240	242·5	+2·5	1·0
300	303·0	+3·0	1·0
360	363·5	+3·5	1·0
420	423·5	+3·5	0·8
480	483·0	+3·0	0·7
540	542·0	+2·0	0·4
600	600·0	0	

This defect is even more marked in some parts of the scale with the ordinary d'Arsonval galvanometer when the zero for no current is at one end of the scale. This instrument, as usually made by Messrs. Carpentier, has a tubular iron core supported from a standard which carries the top suspension of the coil, and the stationary permanent magnets are not provided with pole-pieces. In consequence of the core and the suspension not being supported independently, there is great difficulty in centering the coil relatively to both the core and the stationary magnets ; further, neither the core nor the coil can be readily removed. And partly because of this difficulty of centering the coil, partly because its centre of gravity is generally not in the line of suspensions, and partly because the stationary magnets have no properly shaped pole-pieces, there is (as may be seen from the following sample of the results of many experiments we have made on d'Arsonval galvanometers) an error in different parts of the scale far less regular in its value than in the case of the ordinary reflecting-galvanometer with stationary coil.

Ordinary d'Arsonval Galvanometer.

Deflexion from one end of Scale.	Relative Strength of Current.	Error.	Percentage Error.
0	0	0	
60	59	−1	−1·7
120	118·5	−1·5	−1·3
180	180	0	0
240	240·6	+0·6	+0·2
300	300	0	0
360	359·8	−0·2	−0·06
420	416·5	−3·5	−0·8
480	473·5	−6·5	−1·4
540	537	−3	−0·6
600	600	0	

We find that it is possible, however, to obtain with a d'Arsonval galvanometer a far more exact proportionality between the deflexion and the current than is given in the preceding table, by:—

Supporting the coil independently of the iron core, so that fairly accurate centering becomes possible. This result has been to a certain extent accomplished in the small type of d'Arsonval galvanometer constructed by Messrs. Nalder.

Allowing the coil to hang freely from the top suspension so that the centre of gravity of the coil is always in the axis of rotation. Electric continuity at the bottom of the coil is then maintained by a spiral of fine wire offering little torsional rigidity.

Fitting iron pole-pieces to the stationary magnets, and shaping these pole-pieces so that the rate of cutting lines of force by the moving coil is practically constant.

The following gives the results of tests made on a d'Arsonval galvanometer constructed in this way, each divisional length on the scale being 1·05 millim., so that the total deflexion of 600 divisions represented a motion of the spot of light over about 63 centimetres, or about 25 inches. The deflexion could be read easily to 0·2 of a division, and it will be observed that the greatest want of proportionality is only 0·15 per cent., which occurs with a deflexion of 480 scale-divisions.

Improved d'Arsonval Galvanometer.

Deflexion from one end of Scale.	Relative Strength of Current.	Error.	Maximum Percentage Error.
0	0	0	
60	60	0	
120	119·9	−0·1	
180	179·8	−0·2	
240	239·9	−0·1	
300	299·8	−0·2	
360	360·2	+0·2	
420	420·5	+0·5	
480	480·7	+0·7	0·15
540	540·5	+0·5	
600	600	[illegible]	

To improve the magnetic circuit in d'Arsonval galvanometers some English makers replace the original tubular core by a solid iron core; and Messrs. Jolin of Bristol form the core of a series of short horizontal magnets laid vertically one above the other. A comparison that we have made of the sensibilities of the instruments constructed in these different ways has not shown that the English instruments are superior in this particular respect to the French. This may be, however, due to the fact that the horseshoe permanent magnets employed by the English manufacturers are inferior to those used by Messrs. Carpentier.

The ordinary methods adopted for attaching the suspension wires of the d'Arsonval coil to the supports seem to have been designed with total disregard of the fact that these wires convey the current into and out of the coil. This is particularly serious when a d'Arsonval galvanometer is used as a voltmeter, especially when it is employed to measure a small fraction of a volt, and when therefore the total resistance in the circuit must be small.

In fact we have found that before any reliance can be placed on the indications of a d'Arsonval galvanometer employed in this way, it is necessary to solder all the joints. And instead of trusting to contact through the supports to which the torsional suspension-wires are attached, we have found it desirable to solder wires coming from the terminals of the instrument directly to the hooks to which the suspension-wires are attached. Perhaps the best plan is to solder wires at one of their ends to the terminals of the instrument, and at their other ends to an extension of the torsional suspension-wires.

In devising very low-resistance d'Arsonval galvanometers careful consideration must be given to the suspension-wires which lead the current into and out of the moving coil, since being usually made of German silver they may easily become slightly heated by the relatively large current employed with a very low resistance-galvanometer, and in consequence their elasticity temporarily diminished. This would of course have the effect of making the instrument more sensitive for large currents than for small; but as the sensibility for large currents would, until a limit was reached, increase with the time the current was kept flowing through the galvanometer, such an instrument could not be used for accurate measurement. One method of overcoming this difficulty is to make the suspension of very thin phosphor-bronze strip, such as is employed in the construction of delicate "Ayrton and Perry magnifying-springs." For a thin strip has, for its cross-section, a very small torsional rigidity and a very large radiating surface, and therefore is the very thing to employ when we want a conductor with small torsional rigidity, and which will be very slightly raised in temperature by the passage of a current through it.

D'Arsonval galvanometers, when used as voltmeters, are subject to the error that affects all electromagnetic voltmeters, arising from a variation of the resistance of the coil due to a variation in the temperature of the room; if, however, both the coil and the suspension-wires are made of platinum-silver, a very curious compensating effect is brought about, since the percentage increase of resistance with temperature of platinum-silver is almost exactly equal to the percentage diminution in its torsional rigidity. Hence when a rise of temperature diminishes the current flowing through the instrument for a given P.D. maintained at its terminals and so diminishes the deflecting couple, a proportional diminution in the torsional rigidity of the suspending wires, and therefore in the controlling couple, is produced at the same time. *It is thus possible to construct an electromagnetic voltmeter of one metal only, which has no temperature error.*

VII. *Sensibilities of Different Types of Galvanometers.*

Apart from differences in detail (as, for example, in the size shape, or number of the coils, &c.), galvanometers may be divided into four distinct classes, viz.:—

1. Galvanometers in which the moving system consists of one or more magnetic needles turning about an axis at right angles to the magnetic axes of the needles.
2. Galvanometers in which the needles have a motion of translation along their own magnetic axes, the magnetic needles being drawn into, or pushed out of, coils acting like solenoids.
3. Galvanometers in which the magnetic system is stationary, and the moving system consists of a coil turning about an axis on its own plane.
4. Instruments based on the heating of a conductor by the passage of a current through it.

Type 1 includes tangent-galvanometers, Thomson's reflecting-galvanometers—in fact, so many well-known instruments that further description is unnecessary. In addition to galvanometers with straight magnetic needles, it may be noticed that type 1 also includes instruments containing a bell-shaped horseshoe-magnet; since the bell-shaped magnet is only a device for producing the equivalent of a very powerful and permanent short flat magnet.

We may here mention that the instrument shown in fig. 5 is spoken of in this paper as a galvanometer with "one pair of coils," while those illustrated in figs. 6 and 7 are referred to as having each "two pairs of coils."

Type 2 has been employed by Lord Rayleigh, the Profs. Gray, Mr. Rosenthal, by ourselves, and others. Figs. 8 and 9 illustrate an instrument of this type, constructed by Messrs. Edelmann of Munich, and called a Rosenthal Micro-galvanometer, since this instrument has the peculiarity that the coils into which the ends of the magnet are sucked are extremely small, having an *outside* diameter of only 11 mm. and a thickness of 2 mm. Fig. 9 shows the suspended magnet of the Rosenthal galvanometer as constructed by the makers, the arrangement being non-astatic; but we have also made and used with this galvanometer an astatic combination, constructed as in fig. 10.

Fig. 8.

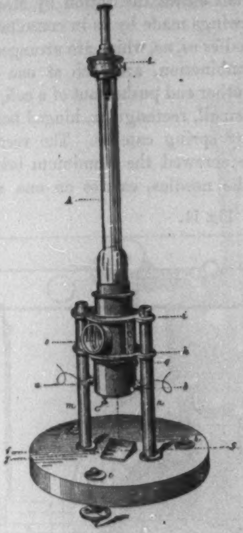


Fig. 9.

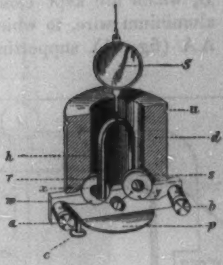
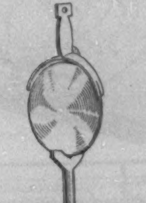


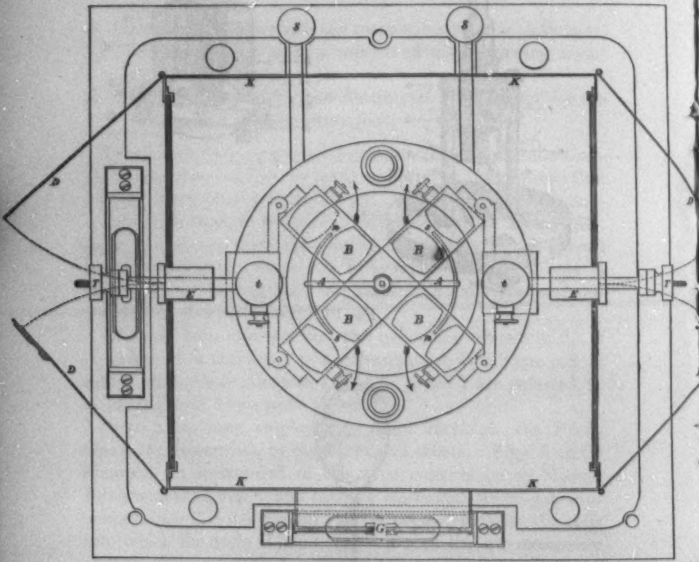
Fig. 10.



Mirror and astatic pair of needles for the Rosenthal Micro-galvanometer.

Figs. 11 and 12 show, in plan and elevation, a ballistic galvanometer constructed for the Central Institution by Messrs. White of Glasgow, from drawings made by us in consultation with Prof. T. Gray. The needles *ns*, *ns*, which are arranged so as to produce an astatic combination, are each at one end sucked into a coil and at the other end pushed out of a coil, the coils being contained in four small, rectangular, hinged boxes, B, which are kept closed by spring catches. The vertical aluminium wire, to which is screwed the aluminium bridge AA (fig. 11), supporting the needles, carries on one side of its lower end a concave mirror O (fig. 12) to produce a spot of light on a scale, and on its other side a truly

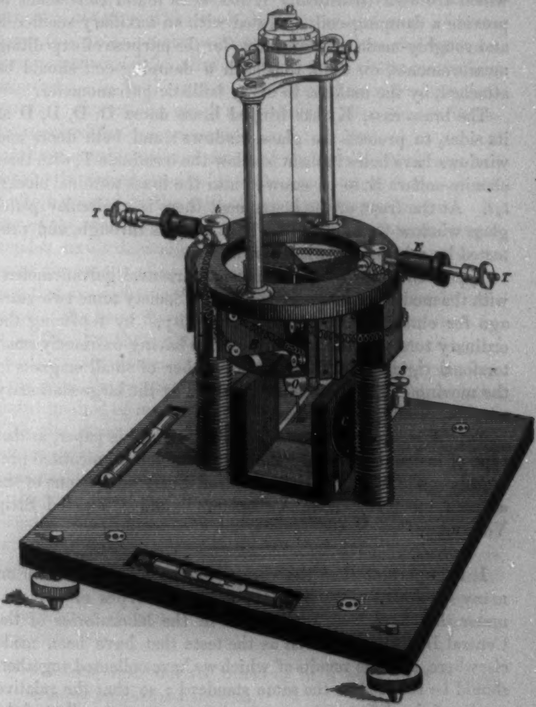
Fig. 11.



Plan of Ballistic Galvanometer.

plane mirror, to be viewed with a telescope, if desired. At its lower end it carries a small magnetic needle, *m*,

Fig. 12.



Perspective elevation of Ballistic Galvanometer with brass case removed.

to be acted on by a pair of coils, C, joined up in series and having their ends led to the terminal screws, *s*, *s*. These coils are quite independent of the main coils of the instrument, and are employed for damping the oscillations of the suspended system. With ballistic galvanometers previously made for us we have not had any such damping-coil attached; but since to all our galvanometers which are used ballistically it has been found convenient to provide a damping-coil, operated with an auxiliary small cell and roughly-made reversing-key, for the purpose of expediting measurements, we think that such a damping-coil should be attached, by the makers, to every ballistic galvanometer.

The brass case, K, has hinged brass doors D, D, D, D at its sides, to protect the glass windows; and both doors and windows have holes in them to allow the terminals T, with their ebonite collars E, to be screwed into the brass terminal blocks, *t*, *t*. At the front of the instrument there is a circular plate-glass window G, to allow the light to pass through, and protected by a brass cap when not in use.

Type 3 includes the well-known d'Arsonval galvanometers, with the modification described to this Society some two years ago for obtaining "invariable sensibility," by replacing the ordinary torsional suspension with one having extremely small torsional rigidity and inserting a number of small magnets in the moving coil, which, being attracted by the large stationary magnet, produce the control.

Type 4 we do not propose touching on in this paper, as that type of instrument formed the subject of communications previously made to this Society by Prof. Perry and by one of the authors on "Hot-Wire Voltmeters" and "Twisted Strip Voltmeters."

It is important that the results of the numerous tests on many specimens of each of the first three types of galvanometer that have been carried out in the laboratories of the Central Institution, as well as the tests that have been made elsewhere, and the results of which we have collected together, should be reduced to the same standard; so that the relative merits of the different types of galvanometer, as well as of the different specimens of each type, should be able to be usefully compared. A satisfactory standard of comparison, however, cannot easily be arrived at; and we are not aware that the question of comparing galvanometers of totally different types has previously received much attention. The "figure of merit" of a galvanometer is sometimes defined as "the amount of current which will produce one division or degree of deflexion"*. But although the figure of merit, so defined, tells us the sensibility of a particular galvanometer with a particular adjustment of the controlling magnet, and with a particular distance of the scale, it gives us no means of comparing this galvanometer with other forms, since this figure of merit can be varied by altering the position of the controlling magnet &c.—in fact, is not a constant of the particular galvanometer. Something much more definite than this is therefore required for our purpose.

A little consideration shows that the sensibility of a galvanometer in actual practice is made up of many factors, some of which depend on the skill of the experimenter himself. In view of the sensibility of any particular galvanometer being a complex result depending on many factors, it is desirable to record the results of experiments on galvanometers in such a form that the influence of each factor can be separately estimated. We have therefore endeavoured to distinguish, as far as possible, the three chief causes that affect the sensibility of reflecting-galvanometers, viz. the arrangement and winding of the coils, the construction of the magnetic system, and the optical method of magnifying the angular deflexion produced. With the same view the question of the susceptibility of various galvanometers to outside magnetic disturbances has been considered separately, since, first, the electrical qualities of the instrument are not altered by such disturbances, and, secondly, their influence is dependent, not only on the kind of instrument, but on the place of test. In the Table that is given later on recording the results of a large number of experiments, we have classified galvanometers in four divisions of decreasing susceptibility to outside magnetic disturbance, comprising:—

Non-astatic instruments working in a *weak* field having *strongly magnetized* needles.

Non-astatic instruments working in a *strong* field and having somewhat *weakly magnetized* needles.

Astatic galvanometers, working in a fairly strong field.

Galvanometers of the d'Arsonval type, working in an extremely strong field.

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* 'A Handbook of Electrical Testing,' by H. R. Kempe.

At the end of the Table are given the constants of a collection of non-reflecting instruments. These instruments, being all of a rougher type, have been simply grouped together at the end of the list; consequently their position on the list has no reference to their sensitiveness or unsensitiveness to outside magnetic disturbance.

The optical sensitiveness of an instrument is dependent on many conditions that have no necessary connexion with the electromagnetic qualities of the instrument; for example, the goodness of the mirror, the brightness of the light, the distance of the scale, &c. are important factors. We have therefore given the number of scale-divisions per micro-ampere or per micro-coulomb, assuming the scale to be placed at a distance from the mirror equal to 2000 scale-divisions. As the scale-distance most generally used is a metre, and as the scale-divisions are frequently in half-millimetres, the conditions assumed are often fulfilled in practice. The sensibilities for a steady current, and for a discharge, are thus directly expressed in angular measure, irrespective of optical magnification.

An important and difficult question to decide is whether the factor of merit of a galvanometer should be stated for a constant periodic time of oscillation of the suspended system, or for a constant controlling-moment for unit angle-deflexion, or for a constant controlling-field irrespective of the magnetic moment of the suspended system. The first condition is the most simple to realize experimentally with different instruments, and the results so obtained are the most convenient for use in subsequent calculations; this condition is, further, the one most easily understood. We therefore give, in columns 3 and 4, the values of D and S, which respectively denote the deflexion per micro-ampere and the swing per micro-coulomb, each in scale-divisions, when the period is 10 seconds and the scale-distance is 2000 scale-divisions. The corresponding values at any other period T are of course $DT^2/100$ and $ST/10$. It would, however, be unfair to rest satisfied with the results given in these tables for the purpose of comparing different galvanometers. For a galvanometer having a large mirror, and intended to be used at a considerable distance from the scale, would appear to be an inferior instrument, because on the one hand the large moment of inertia of the mirror would necessitate the controlling-moment being large to reduce the period to 10 seconds, while, on the other hand, the magnification arising from the much greater distance that separated, in practice, the scale from this particular galvanometer would have no weight in the values given in the columns for D and S. We have therefore given in column 2 the values of M, which are the millimetres deflexion per micro-ampere when the period is 10 seconds and the scale placed as in the ordinary use of the instrument. We have further, in the following way, reduced the numbers to their corresponding values under the condition of *constant controlling-moment*, the condition of constant controlling-field being rejected, because it is inapplicable to instruments of the d'Arsonval type.

The periodic time T equals

$$2\pi\sqrt{\frac{\text{Moment of inertia} \times \sin\theta}{\text{Controlling-moment for a deflexion } \theta}};$$

therefore, in order to obtain the comparative deflexions that would be obtained with the same controlling-moment per unit angle, we must find the deflexions that would be produced with periodic times proportional to $\sqrt{\text{I}}$, where I is the moment of inertia of the suspended system, the values of which for the various galvanometers are given approximately in C.G.S. units in column 6. Hence, to obtain the values in columns 7 and 8, which give numbers respectively proportional to the steady deflexion per micro-ampere and the first swing per micro-coulomb for the same controlling-moments for all galvanometers, we must multiply D by I and S by $\sqrt{\text{I}}$. Now the stability of the zero of any galvanometer depends on the controlling-moment per unit angle of deflexion; hence columns 7 and 8 give numbers proportional to the angular deflexion per micro-ampere and the angular swing per micro-coulomb for the same amount of stability of the zero for all the instruments. The periodic times, however, will be very different for the different numbers either in column 7 or for the different numbers in column 8, the periodic time corresponding with any particular number being equal to $10\sqrt{\text{I}}$ seconds, where I is the moment of inertia given in column 6 for the particular instrument in question. For example, the numbers 3200 and 283, given in columns 7 and 8 for Profs. T. and A. Gray's galvanometer, correspond with a periodic time of $10\sqrt{50}$, or about 70 seconds; while the numbers 542 and 3400, given in the same columns for the 360,000-ohm galvanometer at the Central Institution, correspond with a periodic time of only $10\sqrt{0{\cdot}01}$, or 1 second. The numbers, then, in columns 7 and 8 give a fair comparison between the various galvanometers when the time required to take an observation is unimportant.

The influence of resistance has next to be considered. If two galvanometers differ only in the gauges of wire with which they are wound, and the convolutions are similarly distributed, then the sensibilities should be proportional to the number of turns. If, again, the thickness of the insulating covering bears a constant proportion to that of the wire, then the resistance of the coils would be proportional to the square of the number of turns; so that the values of $D/r^{\frac{1}{2}}$, $S/r^{\frac{1}{2}}$ would be constants for the same pattern of instrument, where D and S are the number of scale-divisions per micro-ampere and micro-coulomb respectively, and r is the resistance of the galvanometer. The proportion in question, however, varies not only for coils of different resistances, but also in the same coil, for several different gauges are generally used in winding the various portions. To get some idea of the relation between resistance and sensibility, we have made tests on galvanometers of the same pattern but of different resistances, the same suspended system of magnets being used successively in each instrument, the controlling magnet being adjusted to produce the same periodic time in each experiment, and, when the galvanometers were reflecting ones, the scale being placed at the same distance from each of the instruments. The following were the results obtained with three astatic reflecting-galvanometers having coils of exactly the same size in each case, but of very different resistance.

Resistance of Galvanometer, in ohms.	D.	$D/r^{\frac{1}{2}}$.	$D/r^{\frac{2}{5}}$.
26	349	68·5	94·4
2457	2160	43·5	95·4
6410	3360	42	101

From this it would appear that, if the only thing altered in a galvanometer be the gauge of wire with which it is wound, the sensibility is more nearly proportional to the resistance raised to the power $\frac{2}{5}$ than to the resistance raised to the power $\frac{1}{2}$.

Another set of experiments was now made with four non-astatic galvanometers, the coils of each of which were made in the same mould, totally different, however, in shape from that employed in constructing the coils of the three reflecting-galvanometers given in the last list. When tested with the same suspended system, the controlling magnet being adjusted to produce the same periodic time in each case, the following results were obtained :—

Resistance of Galvanometer, in ohms.	M, Fraction of one degree per micro-ampere.	$M/r^{\frac{1}{2}}$.	$M/r^{\frac{2}{5}}$.
0·87	0·0149	0·01597	0·0157
4·30	0·0333	0·01604	0·0185
64·23	0·1120	0·01397	0·0246
399·25	0·2700	0·01352	0·033

In this case the sensibility appears to be more nearly proportional to $r^{\frac{1}{2}}$ than to $r^{\frac{2}{5}}$. We have, therefore, in the large table given two columns, one (No. 9) for $D/r^{\frac{1}{2}}$ and one (No. 10) for $D/r^{\frac{2}{5}}$, which represent (according as $r^{\frac{1}{2}}$ or $r^{\frac{2}{5}}$ may be regarded as the more accurate divisor to employ) the deflexion per micro-ampere on a scale whose distance is 2000 scale-divisions for a periodic time of 10 seconds that would be obtained if all the different forms of galvanometers were wound with such a gauge of wire that the resistance of each instrument was 1 ohm. Column 11 gives the swing per micro-coulomb on the same hypothesis, but in this case we have only used $r^{\frac{1}{2}}$ as the divisor.

And whereas columns 9, 10, and 11 give the relative values for the same periodic time, columns 12 and 13 give the deflexion per micro-ampere and the swing per micro-coulomb on a scale whose distance is 2000 scale-divisions for *the same controlling-moment* that would be obtained if all the galvanometers had 1 ohm resistance—the periodic time in any case being equal to $10\sqrt{\mathrm{I}}$ seconds, I being the moment of inertia of the suspended system given in column 6.

Another question to be considered is the factor of merit in connexion with the volume of the coils. As this is important as regards the cost of high-resistance galvanometers, a separate column (5) is therefore given, showing the approximate volume in cubic centimetres occupied by the wire in each instrument, and other columns (14, 15, 16, 17) give the values obtained by dividing this quantity into the various factors of merit. The results of comparison in these latter columns are very interesting. It appears that galvanometers of the Rosenthal or Gray and the d'Arsonval types are by far the most sensitive in proportion to the volume of coil; and we believe that the best way to make a very sensitive galvanometer with a movable magnetic system is to employ several small coils, instead of one or two large ones*, and that the magnets should be horseshoe-shaped, with the line joining the poles vertical, as in Prof. Gray's instrument, but modified so as to give smaller moment of inertia. Such a magnetic system can be made very delicately astatic, and the weakening of the magnets with time does not greatly influence the astaticism. We also conclude from our investigations that the most sensitive galvanometer of all would be one of the d'Arsonval type suitably modified. The poles of the magnet should be very close together, the coil should be very long and narrow, and no stationary iron core should be used inside the coil as in the ordinary d'Arsonval galvanometer. The sensitiveness could be further increased by employing electro-magnets instead of permanent ones to produce the deflecting field, the current flowing round the electromagnet being kept constant by means of an auxiliary reflecting "set-up ammeter" producing an image on the same scale as is used for the electromagnetic d'Arsonval galvanometer. With a "set-up ammeter" the suspended system is supported by means of an almost torsionless phosphor-bronze strip, requiring many twists to be given to it to bring the spot of light on to the scale when the current, that is to be kept constant, is flowing through it. A motion, then, of the spot of light over 100 divisions of the scale corresponds perhaps to a variation of only $\frac{1}{20}$ in this current; and consequently, by means of such an instrument and a suitable adjustable resistance, a current can be kept constant to a very small fraction per cent.

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* Mr. C. V. Boys, F.R.S., in his Cantor Lectures on "Instruments for Measuring Radiant Heat" (April 1889), has also shown that galvanometers with small coils may be made very sensitive (pp. 22, 23).

It might be objected to our conclusions that, in the papers read by Prof. Threlfall before this Society, and published in the 'Philosophical Magazine' for December 1889, he comes to the conclusion that coils used in what we may shortly call the Mudford way (figs. 5, 6, and 7) give greater sensibility than when used in the Gray way (figs. 8–12). But on studying Prof. Threlfall's papers we find that, from various numerical errors, he makes his coils when used as a Gray galvanometer one ninth as sensitive as they really were, and when used as a Mudford galvanometer seventeen times as sensitive as they were; so that Prof. Threlfall's ratio of the sensibility of the Gray galvanometer to the sensibility of the same coils when used as a Mudford galvanometer requires to be multiplied by the very large number 153*.

In order to correct Prof. Thelfall's calculations, the following alterations must be made:—

Phil. Mag., Dec. 1889: Page 465, line 11, *for* "The current was therefore $1{\cdot}26 \times 10^{-7}$ amperes," *read* "The current was therefore $1{\cdot}43 \times 10^{-8}$ amperes."

Same page, line 25, *for* "$2{\cdot}5 \times 10^{-8}$ amperes" *read* "$2{\cdot}8 \times 10^{-9}$ amperes."

Page 469, line 1: "The galvanometer having been brought to a state of sensitiveness of 5 scale-divisions for 10^{-11} amperes, the measurement of the resistance of the sample of sulphur

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* These errors were also noticed by Prof. A. Gray, and referred to in an article in the Phil. Mag. for February 1890.

		1.	2.	3.	4.
Type.	Description.	r.	M.	D.	S.
Thomson. One pair of coils.	Elliott's Tripod (F.T.C.) ‡	0·6	3·8	7·6	4·76*
	Tripod „ ‡	50	102	204	128*
	Elliott's Tripod. Specially light needles †	3860	7550	15,400	9,700*
		6000	15,750	32,000	20,000*
Thomson. Two pairs of coils.	Elliott's Glass Cylinder (F.T.C.) ‡	4600	1220	2440	1520*
	Latimer Clark & Muirhead (F.T.C.) ‡	20,000	8700	10,700	6720*
	Elliott's Stock pattern †	7000	4300	7110	4450*
	„ Large Coil pattern †	12,300	7680	15,200	9550*
	„ „ „	30,000	10710	21,800	13,700*
	„ Special form †	30,000	20,000	40,000	25,000*
	„ Ayrton & Perry ballistic (Japan)	20,000	1070	1430	900
	Langley's Bolometer	20	125	250	156*
Mudford's Thomson. One pair of coils.	Figure 5 (C.I.)	26	347	349	220*
	„ „	2,457	1375	2160	1360*
	„ „	6,410	2720	3300	2110*
	„ †	2086	880	1720	1080*
	Bell Magnet Ballistic †	7800	720	1350	850*
	„ „ „	10,000	640	1200	755*
	Figure 5 (C.I.)	5330	......	2280	1440*
Mudford's Thomson. Two pairs of coils.	Figure 6 (F.T.C.) ‡	700	1720	2970	1860*
	„ (C.I.)	686	1660	1600	1010*
	Variable damping Large Coils (C.I.)	9744	4200	4280	2640
	Figure 7 (C.I.)	360,000	50,000	54,200	34,000*
	„ †	350,000	15,700	31,400	19,700*
Rayleigh, Gray, or Rosenthal.	Figure 8, Non-astatic (C.I.)	44	22·9	25·4	16
	Figure 9, Astatic (C.I.)	44	25	27·7	17·4
	Non-reflecting (C.I.)	5·2	......	9·06	5·7*
	Gray's (Proc. Roy. Soc. 1884, p. 287)	30,000	32	64	40*
	Figures 11 & 12 (C.I.)	4090	54	63	39·2
	Threlfall's (Phil. Mag. Dec. 1889, p. 460)	15680	2·90	3·74	2·35
d'Arsonval.	Jolin's High-resistance (C.I.)	750	137	260	163*
	Carpentier's ordinary	208	286	375	236*
	Jolin's Low resistance (C.I.)	3·8	39	42	26·4*
	Invariable sensibility, Large type (C.I.)	21	190	181	114*
	Nalder's type fitted with Invariable-sensibility device. Pole pieces in (C.I.)	274	......	169	106*
	„ out „	274	......	242	152*
	Carpentier milliamperemeter (C.I.)	183	......	73	45·7*
Flat Coil or Multiplier type.	Single Needle Post-office pattern (C.I.)	800	......	190	119*
	Coils 6 centim. long outside „ 2·7 „ broad „ „ 2·2 „ high „ Winding 0·6 „ thick.	400	......	303	190*
		350	......	146	91·7*
		64	......	125	78·7*
		4·3	......	14·8	9·3*
		4·3	......	35·2	22·1*
		·87	......	6·8	4·27*
		·87	......	16·7	10·5*
	Detector nearly astatic †	2000	......	490	308*
	„ partially astatic †	2000	......	42	26·4*

5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.
V.	I.	DI.	$SI^{\frac{1}{2}}$.	$D/r^{\frac{1}{2}}$.	$D/r^{\frac{3}{4}}$.	$S/r^{\frac{3}{4}}$.	$DI/r^{\frac{3}{4}}$.	$SI^{\frac{1}{2}}/r^{\frac{3}{4}}$.	$D/r^{\frac{3}{4}}V$.	$S/r^{\frac{3}{4}}V$.	$DI/r^{\frac{3}{4}}V$.	$SI^{\frac{1}{2}}/r^{\frac{3}{4}}V$.
......	......	......	......	10	9·4	5·9						
......	·04	81·6	25·6	28·7	42·8	26·9	17·1	5·4				
125	·01	154	970	248	566	356	5·66	35·6	4·53	2·85	0·045	0·29
125	·01	320	2000	413	986	616	9·86	61·6	7·89	4·93	0·079	0·49
......	......	......	......	36	83	52						
......	......	......	......	76	203	128						
30	0·013	92	507	85	205	129	2·68	14·7	6·8	4·3	0·089	0·049
240	0·013	198	1090	137	350	220	4·53	25·2	1·46	0·91	0·019	0·10
240	0·013	284	1560	126	353	220	4·6	25·3	1·47	0·91	0·019	0·10
250	......	......	......	231	648	405	......	......	2·67	1·60		
......	......	......	......	10·5	27·3	17·1						
......	......	......	......	56	76	47						
72	·042	14	45	66·5	94·4	60	3·92	12·2	1·31	0·83	0·055	0·16
72	·042	91	284	43·5	95·5	60	4·00	12·3	1·33	0·83	0·055	0·16
72	·042	142	435	42	101	63	4·25	13·0	1·40	0·87	0·059	0·17
72	......	......	......	37·6	81	51	......	......	1·12	0·71		
......	......	......	......	15·3	37·3	23·5						
......	......	......	......	12·0	30·2	19·0						
72	·042	95·5	295	31·0	74·0	46·6	3·1	8·9	1·03	0·65		
......	......	......	......	112	215	136						
88	·084	134	293	61	118	74	9·81	21·5	1·34	0·84	0·011	0·24
200	·052	222	602	43·5	108	67	5·61	15·3	0·54	0·34	0·028	0·076
236	·01	542	3400	90	324	202	3·24	20·2	1·37	0·86	0·014	0·086
......	......	......	......	53·1	190	119						
0·29	1·5	38	19·6	3·82	5·6	3·5	8·4	4·3	19·3	12·1	29·0	14·8
0·29	0·7	19·4	14·5	4·2	6·1	3·8	4·27	3·2	21	13·1	14·7	11·0
2·0	·21	1·91	2·61	4·0	4·7	2·94	·98	1·35	2·35	1·47	0·49	0·67
51	50	3200	283	0·37	1·04	0·6	52	4·6	0·02	0·012	0·10	0·09
36	17	1070	165	1·0	2·26	1·4	39	6·0	0·06	0·04	1·1	0·17
72	......	......	......	·03	·078	·049	......	......	·00108	·00068		
2·8	3·0	7800	895	9·5	18·4	11·5	55·1	6·3	6·6	4·1	19·8	2·2
......	......	......	......	26	44·3	27·9						
0·6	5·5	230	62	21·5	25	15·5	134	36·3	42	26	225	61
0·96	13	2360	410	39·5	54	33·6	700	122	56	35	730	126
·2	1·5	254	129	10·2	17·7	11·2	27	13·7	88·5	56·0	135	68·5
......	......	......	......	14·6	25·5	16·1						
3·2	25	1820	228	5·4	9·0	5·6	225	28	2·8	1·74	70	8·7
......	......	......	......	6·7	131	82						
......	......	60·6	85	15·1	27·6	17·3	5·5	7·75	1·26	0·79	0·25	0·35
22	·2	29·2	41	7·8	14·0	8·8	2·82	3·94	0·64	0·40	0·13	0·18
......	......	25	35	15·6	23·8	14·9	4·75	6·65	1·08	0·68	0·21	0·30
......	......	2·96	4·1	7·1	8·25	5·18	1·65	2·28	0·37	0·23	0·07	0·10
......	......	7·04	9·9	17·0	19·6	12·3	3·92	5·5	0·85	0·56	0·18	0·25
......	......	1·4	1·9	7·3	7·2	4·53	1·49	2·02	0·32	0·21	0·07	0·09
......	......	3·3	4·7	18	17·9	11·1	3·5	5·0	0·82	0·50	0·15	0·23
......	......	......	......	1·1	23·4	14·7						
......	......	......	......	·09	2·0	12·6						

in question became a tolerably easy matter." On examining, however, the results obtained in the actual measurements with the sulphur (p. 471), we find that the sensibility of the galvanometer was not as above stated, 500,000 divisions per micro-ampere, but 34,200. And, as his scale was 3 metres away from the galvanometer, and his scale-divisions 1 millim., this number 34,200 reduced to our standard becomes 22,800*.

EXPLANATION OF TABLE.

r=resistance in ohms.

M=millimetres deflexion per micro-ampere when the period is 10 seconds, and the scale is placed as in the actual use of the instrument.

D=deflexion in scale-divisions per micro-ampere when the period is 10 seconds, and the scale-distance is equal to 2000 scale-divisions.

S=swing produced per micro-coulomb under the same conditions as in last.

V=volume occupied by the convolutions of wire, in cubic centimetres approximately.

I=moment of inertia of the suspended system, in C.G.S. units approximately.

DI=deflexion in scale-divisions (scale-distance equal to 2000 scale divisions) per micro-ampere for constant controlling moments, and for a periodic time equal to $10\sqrt{\mathrm{I}}$ seconds.

$\mathrm{SI}^{\frac{1}{2}}$=swing per micro-coulomb under the same conditions as in last.

Columns 9, 10, and 11 give the deflexion per micro-ampere, and the swing per micro-coulomb, when the period is 10 seconds and the resistance of each instrument one ohm.

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* Prof. Threlfall says, in a letter published in the Phil. Mag. for June 1890, in reply to these criticisms, that his galvanometer was not in a state of maximum sensibility when used for these experiments on sulphur. Granting this, and looking at the observations given on page 472 of the Phil. Mag., vol. xxviii., from which the constant of the galvanometer was determined, we find that there was considerable vagueness of the zero, amounting in some cases to more than 4 per cent. of the double deflexion, as well as a gradual change in the position of the zero from 105 to 78. If, then, the control were diminished to about $\frac{1}{17}$ of the value it then had, and which would be necessary to give the galvanometer the sensibility claimed for it, the vagueness of the zero would probably become too serious for the instrument to be used for any but the very roughest measurements.

Columns 12 and 13 give the deflexion per micro-ampere and the swing per micro-coulomb, for the same controlling moment, and for the resistance of each instrument equal to one ohm.

Columns 14 and 15 give the deflexion per micro-ampere and the swing per micro-coulomb per cubic centimetre of coil, when the period is 10 seconds and the resistance of each galvanometer one ohm.

Columns 16 and 17 give the deflexion per micro-ampere and the swing per micro-coulomb per cubic centimetre of coil, for the same controlling moment, and the resistance of each galvanometer equal to one ohm. Periodic time of any instrument is 10 $\sqrt{I}$ seconds.

C.I., Central Institution.

F.T.C., Finsbury Technical College.

† The constants of these instruments have been kindly furnished by the makers.

‡ These instruments were tested after being in use for some time and without their needles being remagnetized.

* These numbers are calculated from those given in column 3.

VIII. *Self-Induction of Galvanometer-Coils.*

We have already seen that the sensibilities of two precisely similar galvanometers, which differ only in resistance, cannot easily be predicted from their resistances, since, in consequence of the varying thickness of the insulating covering of the wire, there is no simple connexion between resistance and the number of convolutions of wire occupying a given space. It therefore occurred to us to consider whether, since the self-induction of a coil of a given size and shape depends mainly on the square of the number of convolutions, and but little on the thickness of the wire or of the insulating covering, the square root of the self-induction would be proportional to the magnetic constant of the coil. The following table gives the results of the tests on the three astatic reflecting-galvanometers already referred to :—

Galvanometer.	Resistance, in ohms. r.	Self-Induction, in secohms. L.	Time Constant, in seconds.	D.
A	26	0·052	0·002	349
B	2457	1·75	0·00071	2160
C	6410	4·72	0·00074	3360

D, as before, standing for the number of scale-divisions of deflexion produced by the same current with the same distance of the scale, the same system of mirror and magnets being used in each case, and the controlling magnet adjusted to give the same periodic time of oscillation. The following gives the ratio of the values of D, of the square roots of the self-inductions, of the square roots of the resistances, and of the resistances to the power $\frac{2}{5}$:—

$$\frac{D_B}{D_A} = 6{\cdot}190\,; \quad \sqrt{\frac{L_B}{L_A}} = 5{\cdot}801\,; \quad \sqrt{\frac{r_B}{r_A}} = 9{\cdot}721\,; \quad \left(\frac{r_B}{r_A}\right)^{\frac{2}{5}} = 6{\cdot}167.$$

$$\frac{D_C}{D_B} = 1{\cdot}555\,; \quad \sqrt{\frac{L_C}{L_B}} = 1{\cdot}643\,; \quad \sqrt{\frac{r_C}{r_B}} = 1{\cdot}615\,; \quad \left(\frac{r_C}{r_B}\right)^{\frac{2}{5}} = 1{\cdot}468.$$

$$\frac{D_C}{D_A} = 9{\cdot}627\,; \quad \sqrt{\frac{L_C}{L_A}} = 9{\cdot}526\,; \quad \sqrt{\frac{r_C}{r_A}} = 15{\cdot}70\,; \quad \left(\frac{r_C}{r_A}\right)^{\frac{2}{5}} = 9{\cdot}051$$

From the preceding list it appears that the ratio of the square roots of the self-inductions gives the ratio of the galvanometer-constants of coils, not only much more accurately than the ratio of the square roots of the resistances, but, on the whole, even more accurately than the ratio of the resistances to the power $\frac{2}{5}$.

In view, then, of the importance of knowing the self-induction of galvanometer-coils, we give the results of some measurements made by means of the secohmmeter :—

	Resistance, in ohms.	Self-Induction, in secohms.	Time-Constant, in seconds.
Reflecting-Galvanometer with One Pair of Coils.			
Front coil	3007	3·75	0·00125
Back coil	2830	3·16	0·00112
Front and back coils, joined up as in actual use	5837	9·6	0·00164
Mutual induction of the two coils in position	......	1·35	
Reflecting-Galvanometer with Variable Damping.			
Four coils in series, but separated so as have little Mutual Induction	9680	12·48	0·00129
Reflecting High-Resistance Galvanometer (fig. 7).			
Top front coil	105,600	40·72	0·000386
Top back coil	101,200	39·50	0·000390
Bottom front coil.........	90,000	27·51	0·000306
Front top and back coils joined up in position	207,000	109·8	0·000531
Reflecting Ballistic Galvanometer (fig. 11).			
One Coil only	1003	0·166	0·000166
Flat Single-Coil Galvanometer. (Nobili type.)			
The one Coil	64	0·0324	0·000506

From the above it appears that a coil of the ordinary size and shape used in Thomson galvanometers has a coefficient roughly equal, numerically, to one thousandth of its resistance in ohms. If the coils of the instrument are in series and placed near each other, as in actual use, the coefficient is increased by about half its former amount, owing to mutual induction; so that, for Thomson galvanometers whose resistances vary between 1000 and 10,000 ohms, the time-constant is about 0·0015 second. With very high-resistance instruments the time-constant falls to 0·0005 second, in consequence of the increasing importance of the insulating covering of the wires.

IX. *Effect of Time and Use on Galvanometer-Needles.*

The following give the values of D, the deflexion in divisions of a scale, placed at a distance from the galvanometer equal to a length occupied by 2000 scale-divisions, produced by one micro-ampere, the controlling magnet being adjusted to give a periodic time of oscillation equal to 10 seconds.

Double-Coil Astatic Reflecting-Galvanometer.
Resistance, 686 ohms.

	D.
Needles magnetized and tested the same day	1600
After about two years' use	366
Needles remagnetized and then used for about one year	826

Single-Coil Astatic Reflecting-Galvanometer.
Resistance, 26 ohms.

As obtained from the makers	198
After about three years' use	170
Needles remagnetized and tested the same day	243

Double-Coil Astatic Reflecting-Galvanometer; Variable Damping. Resistance, 9744 ohms.

As obtained from the makers	4200
After about three years' use	3000
Needles remagnetized and tested the same day	4280

Rosenthal Non-astatic Reflecting Micro-Galvanometer.
Resistance, 44 ohms. (Figs. 8 and 9.)

As obtained from the makers	25·4
After four years' use	5·16

The needles of the first and last of these four galvanometers have lost much more magnetism than have the needles of the second and third galvanometers. As regards the Rosenthal instrument it might have been expected, in view of the powerful nature of its magnetic needle (see fig. 9) and of the fact that the current only acts on small projections of this magnet, that the magnetism would not be much diminished by use. But it has to be remembered that, whereas with the ordinary reflecting-galvanometers the zero-position of the needles is that in which the lines of force due to the coil are at right angles to the axes of the magnetic needles, and therefore the needles are not in a position to have their magnetism much altered by the passage of a current, unless that current be very strong, the suspended magnet of the Rosenthal galvanometer will tend to have its magnetism diminished by every current sent through the instrument in such a direction as to push the poles of the suspended magnet out of the coil, since the lines of force of the two coils will pass along the axis of the suspended magnet. Care not having been taken to send the current round the Rosenthal galvanometer, so as to always suck the ends of the magnet into the coils, is probably the explanation of the weakening of its suspended magnets during four years in the ratio of 5 to 1; whereas the falling-off in the sensibility of the first of the four galvanometers in the preceding list in the ratio of 4·3 to 1 during a shorter time is probably due to inferior steel having been used in the construction of its magnetic needles.

In view of the great difference which exists between various specimens of magnet-steel, it is most important that only the very best magnet-steel should be employed in the construction of galvanometers which are intended to have a high sensibility, since the factor of merit of the instrument depends largely on the goodness of the magnetic needles.

Mr. C. V. Boys, F.R.S., thought that the factor of merit of galvanometers should not be given in scale-divisions per micro-ampere under the condition of constant controlling moment. This gave too great an advantage to instruments of the Gray or Rosenthal type. Great sensibility could be obtained by diminishing the moment of inertia of the suspended parts, the practical limit being determined by the trouble due to the silk fibre. Spider lines, when used in place of silk fibres, gave better results. It was possible, by using a good suspending arrangement, to use needles ⅛″ long, and a period of twenty seconds, and to gain a sensibility far greater than those indicated in the paper. Ballistic galvanometers should be made with needles as light as possible. The method proposed of winding the central part of the coil in the opposite sense to the rest would probably not be good, owing to the unevenness of the field produced. The conclusion come to by the authors that D'Arsonval galvanometers of great sensibility should be made with small coils placed in a very strong field, was one he had himself come to, but had finally abandoned, owing to difficulties caused by diamagnetism in the copper and to excessive damping due to Foucault currents.

Mr. Swinburne thought that the factor of merit of a galvanometer should be determined differently, according as it was to be used for the measurement of current, or quantity, or for null methods merely. He saw no great advantage in making practical instruments proportional. The name D'Arsonval should be dropped, as the instrument denoted by it was invented by Varley years ago. He would like to know the relative sensibility of the telephone and the Lippmann galvanometer.

Prof. Fitzgerald stated that Lord Rayleigh had shown that the microscope-method of observing angular deflexions was as sensitive as the ordinary method of mirror and scale, even when only the mirror was used as a pointer, so that if a pointer were attached it would be far more sensitive. The drawback, however, was that it was impossible to distinguish, with the microscope, between lateral displacements of the needles and the angular motion whose measurement was required. To get over this error it was necessary to read both ends of the pointer; but this was hard to do.

LVI. *On* Huygens's *Gearing in illustration of the Induction of Electric Currents. By* Lord RAYLEIGH, *Sec. R.S., Professor of Natural Philosophy in the Royal Institution*[*].

As a mechanical model of the electric machinery at work in the induction of currents Maxwell employed differential gearing; and an apparatus on this principle, designed by him, is in use at the Cavendish Laboratory. Wishing to show something similar in a recent course of lectures, and not having differential gearing at my disposal, I designed more than one combination of pulleys, the action of which should be analogous to that of electric currents. These eventually resolved themselves into Huygens's gearing, invented, I believe, in connexion with the winding of clocks. As this apparatus is easier to understand than differential gearing, and the parts of which it is composed are more likely to be useful for general purposes in a laboratory, I have thought that it might be worth while to give a description, accompanied by an explanation of the mode of action.

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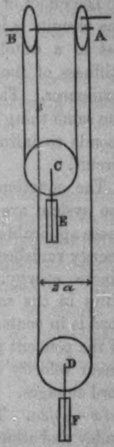
[*] Read May 16, 1890.

Two similar pulleys, A, B, turn upon a piece of round steel fixed horizontally*. Over these is hung an endless cord, and the two bights carry similar pendent pulleys, C, D, from which again hang weights, E, F. The weight of the cord being negligible, the system is devoid of potential energy; that is, it will balance, whatever may be the vertical distance between C and D.



Since either pulley A, B may turn independently of the other, the system is capable of two independent motions. If A, B turn in the same direction and with the same velocity, one of the pendent pulleys C, D rises, and the other falls. If, on the other hand, the motions of A, B are equal and opposite, the axes of the pendent pulleys and the attached weights remain at rest.

In the electrical analogy the rotatory velocity of A corresponds to a current in a primary circuit, that of B to a current in a secondary. If when all is at rest the rotation of A be suddenly started, by force applied at the handle or otherwise, the inertia of the masses, E, F, opposes their sudden movement, and the consequence is that the pulley B turns *backwards*, *i. e.* in the opposite direction to the rotation imposed upon A. This is the current induced in a secondary circuit when an electromotive force begins to act in the primary. In like manner, if A having been for some time in uniform movement suddenly stops, B enters into motion in the direction of the former movement of A. This is the secondary current on the break of the current in the primary circuit.

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* Light wooden laths, variously coloured and revolving with the pulleys, render the movements evident at a distance.

It must be borne in mind that in the absence of friction there is nothing to correspond with electrical resistance, so that the conductors must be looked upon as perfect. The frictions which actually enter do not follow the same laws as electrical resistances, and only very imperfectly represent them. However, the frictions which oppose the rotations of A and B have a general effect of the right sort; but the rotations of C and D, corresponding to dielectric machinery, should be as free as possible.

The effect of a condenser, to which the terminals of one of the circuits is joined, would be represented by a spiral spring (as in a watch) attached to the corresponding pulley, the stiffness of the spring being inversely as the capacity of the condenser. The absence of the spring, or (which comes to the same thing) the indefinite decrease of its stiffness, corresponds to infinite electrical capacity, or to a simply closed circuit.

The equations which express the mechanical properties of the system are readily found, and are precisely the same as those applicable in the electrical problem. Since the potential energy vanishes, everything turns upon the expression for the kinetic energy. If x and y denote the circumferential velocities in the same direction of the pulleys A, B where the cord is in contact with them, $\frac{1}{2}(x+y)$ is the vertical velocity of the pendent pulleys. Also $\frac{1}{2}(x-y)$ is the circumferential linear velocity of C, D, due to rotation, at the place where the cord engages. If the diameter be here $2a$, the angular velocity is $(x-y)/2a$. Thus, if M be the total mass of each pendent pulley and attachment, $\mathrm{M}k^2$ the moment of inertia of the revolving parts, the whole kinetic energy corresponding to each is

$$\tfrac{1}{2}\mathrm{M}\left\{\frac{(x+y)^2}{4}+\frac{k^2}{a^2}\frac{(x-y)^2}{4}\right\}.$$

For the energy of the whole system we should have the double of this, and, if it were necessary to include them, terms proportional to x^2 and y^2 to represent the energy of the fixed pulleys. The reaction between the pulleys A, B depends upon the presence of a term xy in the expression of the energy. We see that this would disappear if $k^2=a^2$; as would happen if the whole mass of the pendent pulleys and attachments were concentrated in the circles where the cord runs. The case discussed above, as analogous to electric currents, occurs when $k^2 < a^2$, a condition that will be satisfied, even without non-rotating attachments, if the cord run near the circumference of the rotating pulleys. The opposite state of things, in which $k^2 > a^2$, would be realized by carrying out masses beyond the groove, and thus increasing the rotatory in comparison with the translatory inertia. In this case the mutual action between A and B is reversed. If when all is at rest A be suddenly started, B moves forward in the *same* direction. Otherwise C and D would have to rotate, and this in their character of fly-wheels they oppose.

Generally, if L, N be the coefficients of self-induction, and M mutual induction, we have (constant factors being omitted)

$$L = N = a^2 + k^2,$$
$$M = a^2 - k^2.$$

In order to imitate the case of two circuits coiled together in close proximity throughout, we must have in the mechanical model $k^2 = 0$; that is, the rotatory inertia of the pendent pulleys must be negligible in comparison with the translatory inertia. Also the energy of the fixed pulleys, not included in the above expressions, must be negligible. If these conditions be satisfied, a sudden rotation imposed upon A generates an *equal* and opposite motion in B.

LVII. *On the Diurnal Variation of the Magnet at Kew.* By W. G. Robson *and* S. W. J. Smith, *of the Normal School of Science and Royal School of Mines*[*].

The multiplication of magnetic observatories makes it extremely desirable that the methods of reducing the observations should be as similar as possible. It is, however, practically impossible that all observatories should determine the diurnal variations of the elements by the elaborate method in use at Greenwich, which, in the case of any one element, necessitates the measurement of the hourly ordinates

[*] Read June 6, 1890.

of the curves at 24 points on all but a very few days of the year under discussion.

Dr. Wild (Rep. Brit. Assoc. 1885, p. 78) advocates the determination of the mean diurnal range from measurements on the comparatively small number of days of very small disturbance, and points out that the inclusion of disturbed days will affect the form of the curve of diurnal range unless the disturbances themselves occur indifferently at all hours of the day and night.

Mr. Whipple, the Superintendent of the Kew Observatory, made a comparison of the methods of Wild and Sabine, and that in use at Greenwich for the years 1870–71–72 (Rep. Brit. Assoc. 1886, p. 71).

His general conclusion was that the difference between the Kew results, as treated by the two methods of Sabine and Wild, was less than the difference between the Greenwich and Kew results in either case.

The range of the differences is given below :—

Mean of three years 1870–72.

	Range of Differences.
Kew (Sabine and Wild)	−0′·3 to +0′·4
Greenwich and Kew (Sabine)...	−0′·6 to +0′·6
Greenwich and Kew (Wild) ...	−0′·9 to +0′·7

It would have been interesting if similar comparisons could have been carried out for more recent years ; but the fact that the Kew magnetic year has not been identical with that used at Greenwich has made such a comparison impossible. The Kew magnetic year has hitherto been from October to October, to admit of the observations being included in the Annual Report made by the Committee to the Royal Society. We understand that in future the civil year will be adopted.

Besides this, some of the Kew results have been obtained not by measuring the hourly ordinates, but by drawing freehand a mean curve among the superposed traces of "quiet day" curves.

Table I.

Year.												Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.	
	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.	20.	21.	22.	23.	24.	
1883.	−1·42	−1·34	−1·42	−1·58	−1·88	−2·18	−2·69	−3·01	−2·51	−0·68	+1·93	+4·29	+5·52	+5·38	+4·15	+2·57	+1·32	+0·32	−0·29	−0·75	−1·24	−1·39	−1·55	−1·53	Greenwich.
	−0·72	−0·77	−0·98	−1·26	−1·80	−2·29	−3·11	−3·77	−3·32	−1·28	+1·50	+3·89	+5·24	+5·06	+3·61	+2·17	+0·90	+0·29	−0·09	−0·35	−0·53	−0·69	−0·92	−0·82	Kew,
1886.	−1·61	−1·46	−1·42	−1·38	−1·57	−1·79	−2·02	−2·07	−1·52	−0·03	+2·17	+4·26	+5·21	+4·92	+3·79	+2·39	+1·14	+0·12	−0·64	−1·18	−1·60	−1·93	−1·99	−1·86	Greenwich.
	−1·29	−1·17	−1·24	−1·57	−1·95	−2·23	−2·59	−2·71	−2·18	−0·47	+1·86	+3·84	+4·87	+4·43	+3·20	+1·91	+0·91	+0·23	−0·04	−0·38	−0·68	−0·81	−0·97	−1·13	Kew,
1887.	−1·47	−1·18	−1·05	−1·15	−1·42	−1·62	−1·88	−1·96	−1·35	+0·20	+2·33	+4·09	+4·91	+4·45	+3·17	+1·83	+0·74	−0·05	−0·71	−1·07	−1·47	−1·55	−1·84	−1·74	Greenwich.
	−1·00	−0·94	−1·04	−1·25	−1·60	−2·06	−2·47	−2·76	−2·01	−0·25	+1·95	+3·93	+4·66	+4·20	+2·69	+1·39	+0·57	−0·04	−0·34	−0·48	−0·57	−0·80	−0·96	−0·84	Kew.

Table II.

Year.												Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.	
	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.	20.	21.	22.	23.	24.	
1870 ...	−0·9	−0·8	−0·7	−0·2	+0·2	+0·2	+0·5	+0·4	+0·6	+0·6	+0·4	+0·1	+0·5	+0·5	+0·4	+0·6	+0·6	+0·2	0·0	−0·1	−0·4	−0·8	−1·1	−1·0	Greenwich and Kew (Whipple)
1871 ...	−0·8	−0·6	−0·8	−0·4	+0·2	+0·7	+0·9	+0·7	+0·7	+0·7	+0·2	+0·5	+0·5	+0·7	+0·8	+0·6	+0·3	+0·1	−0·1	−0·2	−0·9	−0·7	−1·1	−0·9	
1872 ...	−0·9	−0·7	−0·6	−0·5	−0·1	+0·3	+0·5	+0·8	+0·8	+1·0	+0·6	+0·5	+0·7	+0·6	+0·8	+0·5	+0·5	+0·1	−0·1	−0·7	−0·7	−0·9	−1·1	−0·8	
1883 ...	−0·7	−0·6	−0·4	−0·3	−0·1	+0·1	+0·4	+0·8	+0·8	+0·6	+0·4	+0·4	+0·3	+0·3	+0·5	+0·4	+0·4	0·0	−0·2	−0·4	−0·7	−0·7	−0·6	−0·7	Greenwich and Kew (R. and S.)
1886 ...	−0·3	−0·3	−0·2	+0·2	+0·4	+0·4	+0·6	+0·6	+0·7	+0·4	+0·3	+0·4	+0·3	+0·5	+0·6	+0·5	+0·2	−0·1	−0·6	−0·8	−0·9	−1·1	−1·0	−0·7	
1887 ...	−0·5	−0·2	0·0	+0·1	+0·2	+0·4	+0·6	+0·8	+0·7	+0·5	+0·4	+0·2	+0·3	+0·3	+0·5	+0·4	+0·2	0·0	−0·4	−0·6	−0·9	−1·1	−0·9	−0·9	
Mean...	−0·7	−0·5	−0·4	−0·2	+0·1	+0·4	+0·6	+0·7	+0·7	+0·6	+0·4	+0·4	+0·4	+0·5	+0·6	+0·5	+0·4	+0·1	−0·2	−0·5	−0·8	−0·9	−1·0	−0·8	

It was therefore determined to make some further comparison of the different methods of reduction, and at the request of Professor Rücker we have determined the diurnal variation of the declination at Kew for three recent civil years.

The years selected were 1883, a year of maximum sunspots, 1886, and 1887. The mean diurnal range for each year was found by Wild's method—the hourly ordinates for about five quiet days in each month being measured.

The results we obtained have been compared with the corresponding Greenwich results, as published by the Royal Observatory (see Table I.). Thus for the total range for each year at Kew and Greenwich we have the following:—

Total Ranges.

Year.	Kew.	Greenwich.
1883	9'·0	8'·5
1886	7'·6	7'·3
1887	7'·4	6'·9

It is noteworthy that the total range at Kew determined from quiet days is greater than that at Greenwich determined from all days except those of great disturbance. If we may argue from so small a comparison this indicates that storms on the whole diminish instead of increasing the diurnal excursions of the needle. The total range is also apparently decreasing yearly.

The range of differences of the corresponding mean hourly values is:—

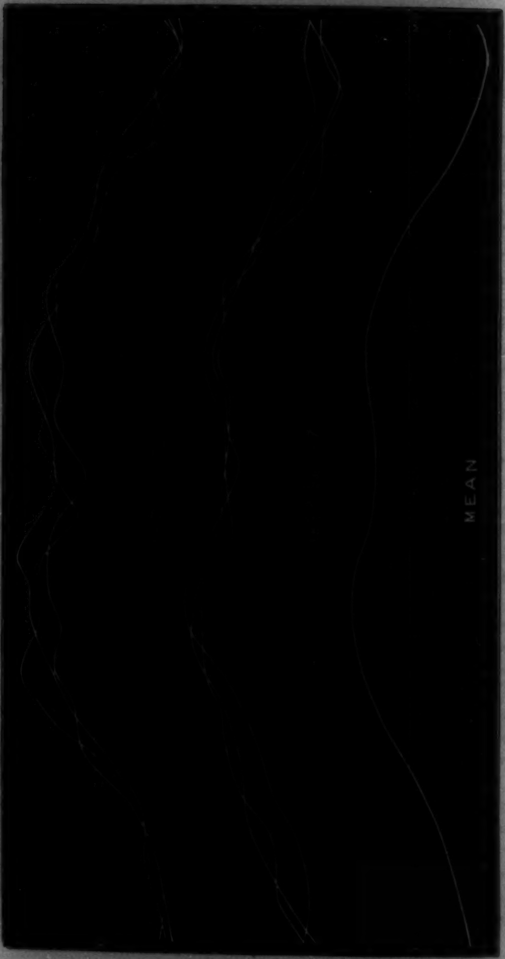
	Range of Differences.	Year.
Greenwich and Kew ...	−0'·7 to +0'·8	1883
" "	−1'·1 to +0'·7	1886
" "	−1'·1 to +0'·8	1887

There is thus in 1883 a difference of 1′·5 in a range of 9′—the percentage being 16·7. For 1886 the difference is 1′·8 in a range of 7′·6—or 23·7 per cent.; while for 1887 the difference is 1′·9 in a range of 7′·4, or 25·7 per cent.

Table II. shows the differences of the hourly readings for the three years just determined, together with the differences for the years 1870, 1871, and 1872, as determined by Mr. Whipple (Rep. Brit. Assoc. *loc. cit.*). The mean of these hourly differences for the six years has been taken (see Table II.). It is noticeable that there is considerable uniformity in the nature of the differences between the mean hourly readings for Greenwich and Kew respectively. Thus, for each year, the Greenwich mean curve falls below that obtained from the Kew magnetograph at about 7 P.M., and remains below it until about 4 A.M., from which hour it is above the Kew curve until it crosses it at about 7 P.M. The minimum ordinate for each curve for each year occurs at about 8 A.M., and the maximum at 1 P.M. The declination has its mean value between 10 A.M. and 11 A.M., and again at between 6 P.M. and 7 P.M. Precisely the same features are to be noticed in the tables given by Mr. Whipple for the years 1870, 1871, and 1872 (Rep. Brit. Assoc. 1886, p. 74); so that it may fairly be assumed that the general nature of the difference between the Greenwich and Kew mean curves from 1870 to 1887 is the same.

The general similarity of the differences is shown clearly by the accompanying curves—the abscissæ of which represent hours, while the ordinates give the mean differences between the Greenwich and Kew readings at these hours. The first set are plotted from the data given in Mr. Whipple's paper. The second set are from the data which we have obtained for the years 1883, 1886, and 1887. The lowest represents the mean of the six curves.

The individual readings do not differ from the corresponding mean to an extent greater than 0′·4. It would therefore seem possible, knowing one set of values for any particular year—Greenwich or Kew—to determine the other set, correct to within four tenths of a minute. As the Kew results are published earlier than the Greenwich ones, it would be possible to calculate approximately the latter from the former.


MEAN

Referring to the reduction of results, Prof. Rücker said that the Stonyhurst observers and Prof. Mascart were willing to adopt Wild's method ; Falmouth, he hoped, would follow suit, and Greenwich had been asked to publish their results in both ways.

Mr. Whipple said that before recording-instruments were available, and the numbers were obtained from separate experiments, the labour involved was considerable, and a single large disturbance or magnetic storm might vitiate the result of a whole year's work. Methods were therefore adopted to to eliminate these disturbances ; of these, that used by Sabine may be particularly mentioned. Although declination-records have now been obtained for a considerable number of years, the cause of the variations still remains unknown. They do not seem to be dependent on temperature or on astronomical facts. He considered it valuable to obtain magnetic data from different parts of the earth, but comparisons were only possible when all are published on the same plan. This, he hoped, would result from the efforts of Professors Rücker and Adams. When this is accomplished, the observations on magnetic force will need treatment ; the work will be laborious, and the aid of volunteers like Messrs. Robson and Smith would be of great service.

Prof. W. G. Adams said he was glad to see the satisfactory nature of the work which had just been brought before the Society. Uusually the mass of figures to be dealt with was so large that the mere reduction was a great undertaking. If, however, the difference between results obtained by the Greenwich and Wild's method was not more than 0′·4, it may be possible to make out the causes of the variations from observations reduced on Wild's plan. He himself would put more faith in horizontal-force observations, and wished they could be worked out by some ready method. He hoped the one adopted in America, of obtaining mean curves by photography, may prove satisfactory.

Prof. Perry asked if a machine could not be made to do the work.

Mr. Whipple said such machines had been used by the Meteorological Office, but they were so elaborate and expensive that clerical work was just as cheap. The method of photographing mean curves had been tried at Kew, but it was open to the objection that accidental disturbances such as those produced by the movement of iron in the vicinity and the approach of cabs &c. were not eliminated.

Mr. Boys, referring to the use of integrators, said that for a harmonic analyser his disk-cylinder pattern was preferable to the ball disk-cylinder integrators of J. Thomson, for it is much cheaper and has less inertia.

The President said the movement initiated by Prof. Rücker would be of great service if it resulted in the numbers obtained at the various magnetic observatories being published in the same way. It was a great advantage to have such men, who were not permanently attached to an observatory, to take up the subjects and suggest improvements. The heads of such institutions were usually too much employed in making the necessary reductions to have time for devising improved methods. In his opinion greater freedom should be allowed to the chiefs of observatories, for it should be borne in mind that the object of observations is not to produce volumes of figures but to increase our knowledge.

Referring to the reduction of observations, he thought the voluntary services of senior physical students should be more generally accepted, and to this end he suggested that properly recommended persons should be allowed to spend some time in observatories as honorary assistants. This would be of great use to the students themselves and an advantage to the observatories, for the reduction of observations could then be expedited. As regards the accidental disturbances referred to by Mr. Whipple, he contended that regulations should be adopted to render them impossible.

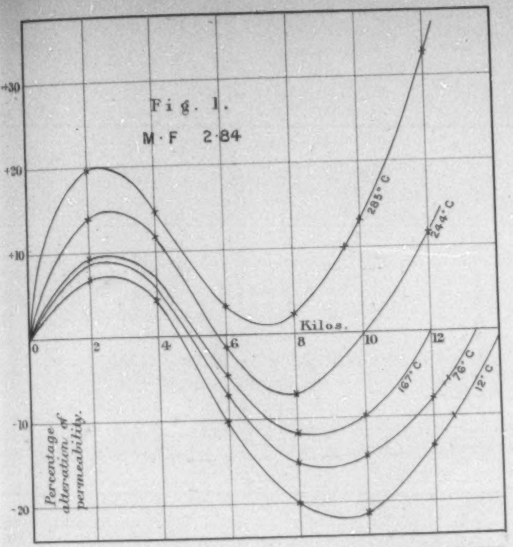


Fig. 1.
M·F 2·84

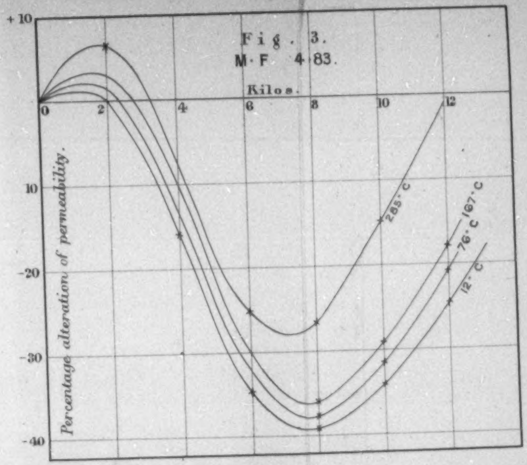


Fig. 3.
M·F 4·83.

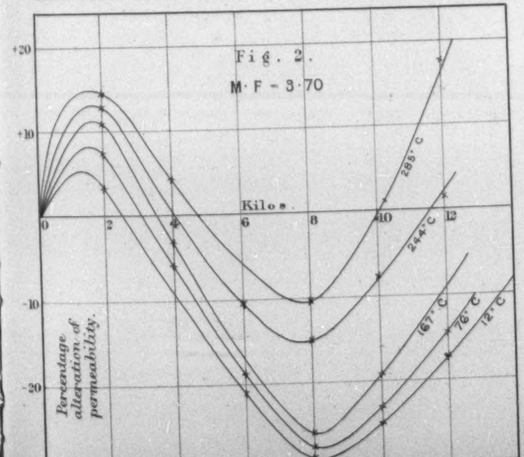


Fig. 2.
M·F – 3·70

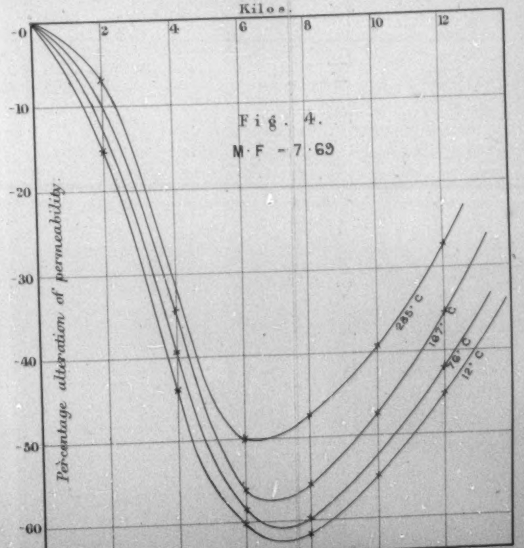


Fig. 4.
M·F – 7·69

Mintern Bros. lith.

LVIII. *The Effect of Change of Temperature on the* Villari *Critical Point of Iron**. *By* HERBERT TOMLINSON, *F.R.S.*†

[Plates XII. & XIII.]

THIS paper must be regarded as a continuation of one previously communicated to the Physical Society‡ on the Villari Critical Points of Nickel and Iron. As before, only the temporary magnetization was studied, and the mode of conducting the observations was also the same with the addition that the effect of loading was tested not only at the temperature of the room but also at various temperatures extending to 285° C. The temperature was ascertained from the resistance of a coil of platinum wire wound double and embracing the same part of the iron wire as the secondary coil; the temperature-coefficients of the platinum coil had previously been very carefully determined§. The iron wire used was 1 millim. in diameter; it had been thoroughly annealed, and the loads employed were not sufficient, even at the highest temperature, to produce any very sensible permanent extension. As a preliminary the wire was heated to 300° C., tested with all the loads and magnetizing forces to be presently mentioned, and again tested when cooled to the temperature of the room. This treatment was repeated several times until constant results were obtained at both temperatures; the observations recorded in figs. I.–VI. were then made. In these figures the abscissæ represent the number of kilogrammes in the load and the ordinates the percentage alteration of permeability at the temperature indicated on each curve, + signifying increase and − decrease of permeability. The Villari critical point is the value of the magnetizing force for which loading does not affect the permeability and may be ascertained by noting the point at which each curve cuts the line of loads; it varies with the value of the load and with the temperature. In some instances the curves cut the line of loads *twice*, and there are consequently *two* loads which for the magnetizing forces pertaining to the curves have no effect on the temporary magnetization. In the following table is given the load (or loads if there are two) which for each particular magnetizing force and temperature is without effect on the permeability.

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* Read June 6, 1890.

† The author expresses his thanks to the Elizabeth Thompson Science Fund for aid in this research.

‡ *Ante*, p. 367.

§ For further information concerning the platinum coil and the primary and secondary coils, see Proc. Phys. Soc. vol. ix. p. 181.

TABLE I.

Magnetizing force in C.G.S. units.	Load for which the permeability is the same as for the unloaded wire. Temp. 12° C.	Ditto. Temp. 76° C.	Ditto. Temp. 167° C.	Ditto. Temp. 244° C.	Ditto. Temp. 285° C.
2·84	4·6	4·8	5·1 and 12·0	5·8 and 10·1	
3·70	2·5	3·2	3·6	4·2 and 11·5	4·7 and 9·9
4·83	1·8	2·5	2·7	not tested.	3·1 and 12·3

It will be seen from Table I. and from figs. I.–IV. that the magnetizing forces 2·84, 3·70, 4·83, and of course all intermediate forces, are Villari critical values. If we regard only the *first point* where each curve cuts the line of loads, we see that at all temperatures the Villari value *increases* as the load *decreases* and, for a given load, increases with the temperature. If, on the contrary, we consider the *second point* of cutting the line of load, we find the critical value *increases both with the load and with the temperature**.

It will be seen from fig. I. that as the temperature rises the two points of cutting the load-line creep nearer and nearer together until for a temperature a little below 285° C. they coincide, and the curve osculates the load-line; whilst for 285° C. and for higher temperatures the magnetizing force 2·84 ceases to be a Villari point for any load. When the

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* The effect of change of temperature on the critical value is therefore in the same direction for both points of cutting the line of loads, *i. e.* in both cases the Villari value is increased by rise of temperature.

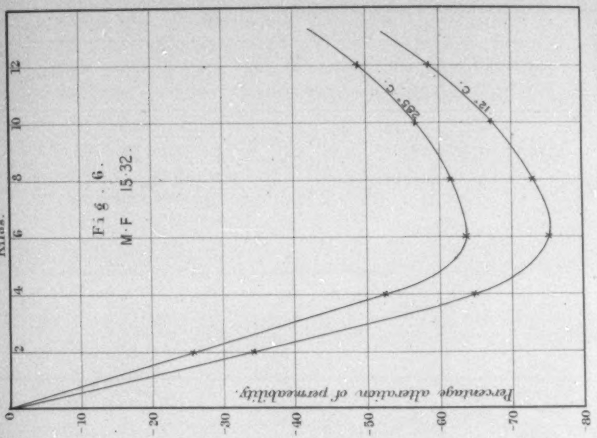

Fig. 6.
M·F 15·32.

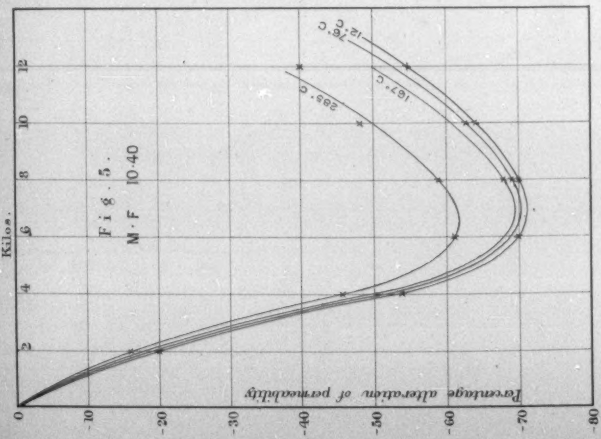

Fig. 5.
M·F 10·40

Mintern Bros. lith.

magnetizing force reaches the value 7·69 (fig. IV.) both points of cutting the load-line vanish, at any rate when the loading is not carried beyond 12 kilos*, and for magnetizing forces beyond 7·69 (figs. V.–VI.) Villari critical values become impossible.

In all cases rise of temperature increases the alteration of the permeability in the positive direction and diminishes it in the negative direction ; but the diminution in the latter case is greater than the increment in the former, so that the summits of the two bends in the curves become more and more nearly on the same level.

The percentage alteration of permeability was calculated as follows :—

If P_0 and P_n represent the permeability with no load and a load of n kilogrammes respectively, the percentage alteration is

$$\frac{100\,(P_n - P_0)}{P_0} \text{ or } \frac{100\,(P_0 - P_n)}{P_0},$$

according as the loading produces increase or decrease of permeability. Table II. gives the values of P_0 for the various magnetizing forces.

TABLE II.

Magnetizing force in C.G.S. units.	Temporary permeability in C.G.S. units of the unloaded wire at 12° C.	Ditto at 76° C.	Ditto at 167° C.	Ditto at 244° C.	Ditto at 285° C.
2·84	301	306	338	353	385
3·70	325	340	383	385	405
4·83	347	366	413	......	461
7·69	478	483	548	......	557
10·40	528	573	638	......	675
15·32	522	......	....	......	617

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* The second point could, however, be probably reached by taking the load to higher values.

LIX. *Notes on Secondary Batteries.* *By* J. H. Gladstone, *Ph.D., F.R.S., and* W. Hibbert, *F.I.C.*[*]

Eight years ago, one of us, in conjunction with the late Mr. Tribe, published a series of papers on the Chemistry of Secondary Batteries[†]. Since that time accumulators have undergone many improvements in detail, but they still present some unsolved problems of a more or less subtle character. In this paper we give an account of some experiments undertaken to test points which have arisen in the last few years.

Action of Sodium Sulphate.

In May 1889 Mr. Preece delivered a lecture at the Society of Arts dealing generally with his experience of Secondary Batteries Among the topics treated was the influence of the composition of the electrolyte on the working of the cell, and it was stated that the addition of sodium sulphate to the acid solution was of very considerable advantage. Mr. Preece determined the proportions of acid and sulphate which gave the best result in a year's working, and found that a comparatively small quantity of the salt was sufficient to produce the effect.

We have determined the percentage composition of the liquid from a sample kindly furnished by Mr. Preece, and find it to be as follows:—

Sodium sulphate (Na_2SO_4) .	0·81
Sulphuric acid (H_2SO_4) . .	29·39
Water	69·80
	100·00

The specific gravity at ordinary temperatures is about 1·23[‡].

When used in secondary batteries instead of the usual acid, it is stated that there is less buckling of the plates and little or no scaling. The chief benefit, however, is stated to be that the sodium salt diminishes the chance of objectionable sulphating in the cell, and where such sulphating occurs, enables it to be removed by a charging current much more readily than when sulphuric acid alone is employed.

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[*] Read June 20, 1890.

[†] Nature, 1882 and 1883, vols. xxv. and xxvi.; afterwards published by Macmillan.

[‡] In making this liquid, it would be best to add together 68 parts (by weight) of water, 30 of strong sulphuric acid, and 2 parts of crystallized sodium sulphate ($Na_2SO_4 . 10H_2O$).

Mr. Preece did not explain the beneficial action of the sodium sulphate, but in the discussion following the lecture Sir F. Abel threw out the idea that it was "probable that a double salt of sulphate of lead and sodium was formed which was soluble, whereas the sulphate of lead was comparatively insoluble."

We have made some experiments to test the value of this suggestion. It may be well, however, to note that lead sulphate is so nearly insoluble in dilute sulphuric acid that comparatively large errors are easily possible.

We took advantage of the greater solubility of lead sulphate in strong acid. Measured volumes of this were added to water, the precipitate formed allowed to settle, and the lead salt remaining in solution determined by precipitation as sulphide.

The mean results are :—1 part of lead sulphate dissolves in 105,000 parts of the acid mixed with water, in 90,000 parts of a similarly diluted acid containing 5 per cent. sodium sulphate, and in 107,000 parts of Mr. Preece's solution.

As these numbers deal with such minute quantities we lay no stress on their absolute value. Their relative values are more useful, and evidently the addition of the sodium salt has not materially increased the solubility of lead sulphate.

This conclusion was confirmed by experiments of another type. To equal quantities of a dilute acid and a similar acid containing sodium sulphate we added a few drops of strong acid saturated with lead sulphate. Precipitates were formed in both cases, but it was possible so to adjust the number of drops that the precipitate did not appear for an appreciable time. We could therefore see in which it appeared first. In many experiments and many variations of procedure, we nearly always obtained earlier precipitation in liquids containing sodium or magnesium sulphates than in those consisting simply of dilute acid. We judge therefore that lead sulphate is not more soluble in these liquids, and that the explanation of the action of sodium sulphate must be sought in some other direction.

Mr. Swinburne has suggested that, as lead sulphate is dissolved by sodium hydrate, the action may be due to that hydrate being produced by electrolysis of the sodium sulphate. Experiment showed that in a simple solution of neutral sulphate of soda, a paste made of equal parts of red lead and sulphate of lead was more readily reduced than in dilute sulphuric acid. It was also easy to detect sodium hydrate on the cathode by red litmus-paper. But this last result was no longer obtained when the sodium-sulphate solution contained sulphuric acid in the proportion of 1 volume to 10. The acid present interferes with such a simple test, and we sought for information on the point by determining the ratio of acid to sulphate of soda in the pores of the spongy lead, and comparing it with the ratio of the same substances in the body of the liquid. Preliminary experiments indicated the following as the best course to pursue.

A strip of spongy lead was used as cathode in a liquid somewhat similar to that analysed above. The ratio was $\frac{H_2SO_4}{Na_2SO_4}=30$. A current was passed for three hours, falling slowly from about 2 amperes to 1·5. The strip was then taken out and placed in 120 cubic centim. of distilled water and allowed to diffuse for 40 minutes. It was then placed in fresh distilled water for 18 hours. The forty minutes' diffusate gave a ratio of $\frac{H_2SO_4}{Na_2SO_4}=25$, only a little lower than that shown by the original liquid. The 18 hours diffusate gave a ratio $\frac{H_2SO_4}{Na_2SO_4}=11{\cdot}8$. Evidently, then, the portions of the liquid which diffused last were much richer in sodium salt than the earlier ones.

This would show that a large proportion of sulphate of sodium is formed in close contact with the lead sulphate, and we have already seen that this is favourable to the reduction of that body.

But there is another consideration. It is well known that the spongy lead formed on one of the plates in a secondary battery is to some extent acted on by the liquid.

It occurred to us that this direct chemical action between lead and sulphuric acid might be influenced by sodium sulphate. We therefore placed a quantity of red lead on a sheet of lead and reduced it electrolytically. The sheet was then cut into small pieces and divided into two equal quantities of about 20 grammes each. One half was put into a flask containing 1 volume of acid to 6 of water; the other half was put into a similar acid with the addition of 5 per cent. of sodium sulphate. Hydrogen gas was evolved from both flasks at the ordinary temperature. The following Table shows the results:—

Time, in days.	Hydrogen collected, in cubic centimetres.			
	From Acid.		From Acid containing Na_2SO_4.	
	Per diem.	Total.	Per diem.	Total.
1	160	160	116	116
2	125	285	91	207
3	120	405	87	294
7	75	695	54	512

It is evident that the sodium sulphate exercised a protecting influence on the lead.

In another experiment with a solution containing as small a proportion of sodium sulphate as that used by Mr. Preece, the protective influence was still observable, though much less marked.

These results support the idea that part of the effect produced by sodium sulphate lies in its power to diminish the local actions arising between the acid and the different parts of the plates.

It is not unlikely that this chemical action has something to do with the difficulty experienced in electrolytically reducing pure lead sulphate when that substance unmixed with other compounds is simply immersed in dilute acid; for at the moment of incipient decomposition the lead must be very finely divided, with its affinities in their most active state.

Sub-sulphate of Lead.

In a paper read before the Royal Society[*], Dr. Frankland doubts whether the lead sulphate formed and decomposed in the cell is the ordinary white sulphate known to chemists, and has studied the action of dilute acid on litharge and on minium. From litharge he obtained a buff-coloured compound, for the composition of which he gives the formula $(SO_3)_3(PbO)_5$. From red lead he obtained a brownish-red compound, $S_2Pb_3O_{10}$. Dr. Frankland thinks it highly probable that one or other of these salts takes part in the electrolytic processes of the storage battery. He remarks, however, very truly that the working conditions in a battery are very different from those of his experiments.

With reference to these results we have made some observations.

Buff-coloured body from Litharge.—The formula $(SO_3)_3$ $(PbO)_5$ can be written $2PbO + 3PbSO_4$. In other words, the substance may either be a true sub-sulphate, or a mixture of the ordinary sulphate and oxide of lead. When attempting to prepare some of this compound, preliminary results suggested the following experiment.

Known quantities of litharge and sulphuric acid were digested together in a flask. From time to time the flasks were shaken vigorously and a small quantity of the clear liquid subtracted. The strength of the acid was carefully determined; and from this, together with the known volume of acid, it was easy to find how much acid had combined with the lead oxide. The curves given below, which are drawn through the actual points of observation, show the course of the action for 28 days, the ordinates giving the acid absorbed, and the abscissæ the time.

In Exp. I., 33·41 grams of litharge and 150 cubic centim. of 1 to 5 acid were used. This liquid contained 43·38 grams of sulphuric acid.

In Exp. II., 39·73 grams of litharge and 150 cubic centim. of 1 to 6 acid $= 36{\cdot}37$ grams H_2SO_4 were used.

In Exp. III., 39·92 grams of litharge and 150 cubic centim. of 1 to 10 acid $= 25$ grams H_2SO_4 were used.

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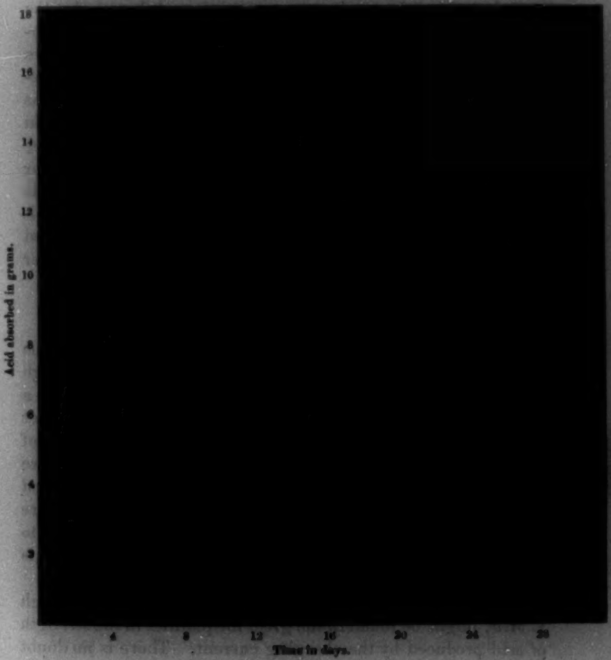
[*] Proc. Roy. Soc. 1888.

The acid required for total conversion of the litharge into sulphate would be,

In Exp. I. = 14·7 grams.
In Exp. II. = 17·45 „
In Exp. III. = 17·54 „

Of course only three fifths of these quantities would have been required to form the compound $(SO_3)_3(PbO)_5$.



The salt gradually became white in colour. It is evident that in each case the action was slow and progressive, without any sudden change of rate to mark the formation of a sub-salt.

At a later date we stopped the experiments and analysed the substances produced in Exps. I. and II.

In I. we found 31·26 per cent. of SO_4, which indicates the presence of 98·6 per cent. $PbSO_4$ and 1·4 per cent. of PbO.

In II. we found 31·67 per cent. of SO_4, which indicates 99·5 per cent. $PbSO_4$ and 0·5 per cent. of PbO.

The general outcome is clearly in favour of the idea that the action is a continuous one, which will only finish when the whole of the litharge is converted into sulphate. As the experiment proceeds there is an increasing proportion of insoluble sulphate present, which no doubts acts as a protective coat for the unchanged litharge, and thus renders the transformation less rapid. We found in fact that the action was greatly hastened if we continually broke up the substance in a mortar.

We see therefore no reason to doubt the view put forth as far back as January 1882, that the substance produced in the voltaic reaction is ordinary sulphate of lead, a substance which is easily oxidated by means of the current, and is reduced without serious difficulty, especially when broken up by better conducting bodies.

High Initial Electromotive Force.

During the prosecution of the experiments described in the preceding pages our attention was again drawn to the high electromotive force which, as is well known, is found in a secondary battery for a short time after the charging current has been stopped. So far as we know, the explanations of this curious phenomenon which have been hitherto given are not looked upon as satisfactory. These explanations depend on the presence and action of electrolytic gases, which are either supposed to be occluded by the electrodes, or else to form secondary products differing from the ordinary elements in much the same way as ozone differs from oxygen.

We thought it probable that the chief cause of the high E.M.F. might be found in the great inequalities in the strength of acid produced by the charging current. There is no doubt that the acid becomes much stronger in the immediate vicinity of the peroxide plate. The strong and weak acid thus produced at the positive and negative plates respectively, being contained in spongy masses of material, require some time to diffuse, and the normal E.M.F. of 2 volts cannot be attained until diffusion has produced uniformity in the strength of the acid. We put this idea to experimental test.

Most of the observations were made on a small experimental cell in which were:—

(1) a negative plate consisting of a strip of lead (1 inch × 3) covered with spongy lead;

(2) a positive plate made of similar lead covered with peroxide;

(3) an electrolyte of dilute acid containing 18·5 per cent. H_2SO_4 (= 1 volume of acid to about 9 of water).

We also provided a small porous earthenware jar filled with stronger acid, into which we could introduce the peroxide plate when desired.

In the first series of experiments the plates were charged in the uniform electrolyte by a current of nearly half an ampere, and after breaking the battery-circuit, observations were taken of the E.M.F. by the condenser method.

Time, after stopping the charging current.	Electromotive force, in volts.			
	Exp. I.	Exp. II.	Exp. III.	Exp. IV.
At once	2·58	......	......	2·58
5 seconds	......	......	......	2·28
1 minute	2·16	2·18	2·17	2·14
2 minutes	2·13	2·17	2·16	2·12
5 "	2·06	2·10	2·10	2·06
10 "	2·01	2·04	2·02	2·05
15 "	2·00	2·01	2·01	
Peroxide plate now put into the jar of stronger acid.				
Time after change of acid round peroxide.	Acid 34 per cent.*	Acid 34 per cent.*	Acid 34 per cent.*	Acid 58 per cent.*
½ minute	2·05 volts.	2·06 volts.	2·05 volts.	2·16 volts.
1 "	2·07 "	2·09 "	2·09 "	2·20 "
2 "	2·07 "	2·12 "	2·12 "	2·21 "
4 "	2·09 "	......	......	2·24 "
5 "	2·10 "	2·13 "	2·13 "	

From this Table it is evident that the rate of fall in the various experiments is fairly uniform, and also that an increase in the strength of the acid round the positive plate (peroxide) is able to give a large increase in the E.M.F. of the cell. We further see that the greater portion of the fall in E.M.F. takes place in the first few seconds after stopping the charging current. This we should expect if the action depends on differences of density in acid, for these differences will be greatest and diffusion most marked in the early stages. In the following set of experiments the spongy lead stood in 18·5 per cent. acid, while the porous jar containing the peroxide plate was filled with acid whose strength in Exps. I. and II. was 34 per cent., and in Exp. III. 58 per cent. The cell thus arranged was charged as before, and after stopping the charging current observations were taken of the E.M.F.

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* These strengths refer to the acid in the porous jar. That in the general body of the cell remained as before—18·5 per cent.

Time, after stopping the charging current.	Electromotive force, in volts.		
	Exp. I.	Exp. II.	Exp. III.
At once	2·61	.....	2·61
5 seconds	......	......	2·44
1 minute	2·22	2·24	2·33
2 minutes	2·21	2·23	2·31
5 "	2·18	2·21	2·28
10 "	2·17	2·20	2·28
15 "	2·14	2·16	2·27
25 "	......	......	2·27
30 "	......	......	2·25

From this Table it appears that the rate of fall is very much slower than before. This is especially marked in Exp. III., where the strength of the acid in the jar is nearer the possible maximum producible in the interstices of the spongy materials.

It may be worth notice, that the value of the E.M.F. to which the cells finally *rise* in Exps. I., II., and III. of the first series is very nearly the same as that to which they *fall* in the first two experiments of the second series. If the rise is due, as we think, to the diffusion inwards of the stronger acid, some such agreement might be expected.

We therefore regard the abnormal E.M.F. as due to the inequality of acid strength, and its gradual disappearance as due to equalization of strength produced by diffusion.

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PROCEEDINGS

AT THE

MEETINGS OF THE PHYSICAL SOCIETY

OF LONDON.

SESSION 1888-89.

February 11th, 1888.

Prof. A. W. Reinold, President, in the Chair.

The following communications were made :—

"On the Limit of Refraction in relation to Temperature and Chemical Composition." By the Rev. T. Pelham Dale.

"Note on the use of the term Resistance in the description of Physical Phenomena." By Mr. R. H. M. Bosanquet.

February 25th, 1888.

Prof. A. W. Reinold, President, in the Chair.

Mr. H. F. Newall was elected a Member of the Society.

The following communications were made :—

"Note on the Efficiency of Incandescent Lamps with Direct and Alternating Currents." By Profs. W. E. Ayrton and J. Perry.

"Observations on the Height, Length, and Velocity of Ocean Waves." By the Hon. Ralph Abercromby.

"On the Temperature at which Nickel begins suddenly to lose its Magnetic Properties." By Mr. **Herbert Tomlinson**.

"Experiments on Electrolysis." By Messrs. W. W. **Haldane Gee**, H. **Holden**, and C. H. **Lees**.

March 10th, 1888.

Prof. A. W. **Reinold**, President, in the Chair.

Mr. **Charles** V. **Burton** was elected a Member of the Society.

The following communications were made:—

"On a Compact Form of Reflecting-Galvanometer, Lamp, and Scale." By Mr. G. L. **Addenbrooke**.

"On the Measurement of the Power supplied to the Primary Coil of a Transformer." By Mr. E. C. **Rimington**.

"On the Magnetic Circuit of Dynamo Machines." By Profs. W. E. **Ayrton** and J. **Perry**.

"Note on the Employment of an Electro-dynamometer for determining the Difference of Phase of two Harmonic Currents of Electricity." By Mr. T. H. **Blakesley**.

April 14th, 1888.

Mr. **Shelford Bidwell**, Vice-President, in the Chair.

The following communications were made:—

"On the Variation of the Coefficients of Induction." By Mr. W. E. **Sumpner**.

"Experiments on Soap Bubbles." By Mr. C. V. **Boys**.

April 28th, 1888.

Prof. A. W. **Reinold**, President, in the Chair.

The following were elected Members of the Society:—

Mr. A. W. **Slater** and Mr. W. H. **Snell**.

The following communications were made:—

"On Electromotive Force by Contact." By Mr. C. V. **Burton**.

"On a Theory concerning the sudden loss of Magnetic Properties in Iron and Nickel." By Mr. **Herbert Tomlinson**.

"Note on the Graphic Treatment of the Lamont-Frölich for Induced Magnetism." By Prof. S. P. **Thompson**.

May 12th, 1888.

Prof. A. W. Reinold, President, in the Chair.

The following were elected Members of the Society:—

Mr. Henry H. Cunynghame and Mr. G. L. Addenbrooke.

The following communications were made:—

"Note on the Conditions of Self-Excitation in a Dynamo-Machine." By Prof. S. P. Thompson.

"Note on the Conditions of Self-Regulation in a Constant Potential Dynamo-Machine." By Prof. S. P. Thompson.

"On Magnetic Lag, and the Work lost due to Magnetic Lag in Alternating-Current Transformers." By Mr. T. H. Blakesley.

"On a Simple Apparatus for the Measurement of the Coefficients of Expansion by Heat." By Profs. W. E. Ayrton and J. Perry.

May 26th, 1888.

Mr. Shelford Bidwell, Vice-President, in the Chair.

Mr. S. O. Roberts was elected a Member of the Society.

The following communications were made:—

"Note on the Governing of Electromotors." By Profs. W. E. Ayrton and J. Perry.

"On the Formulæ of Bernouilli and Haecker for the Lifting-Power of Magnets." By Prof. S. P. Thompson.

"Experiments on Electrolysis.—Part II. Irreciprocal Conduction." By Messrs. W. W. Haldane Gee and H. Holden.

June 9th, 1888.

Prof. A. W. Reinold, President, in the Chair.

Mr. James Livesey Howard was elected a Member of the Society.

The following communications were read:—

"On the Analogy between Dilute Solutions and Gases as regards Gay-Lussac's and Boyle's and Avogadro's Laws." By Prof. van't Hoff. Presented by Prof. W. Ramsay.

"On a Method of comparing very unequal Capacities." By Dr. A. H. Fison.

"On a new form of Lantern." By Mr. W. Lant Carpenter.

"Note on some Additions to the Kew Magnetometer." By Profs. T. E. Thorpe and A. W. Rücker.

June 23rd, 1888.

Prof. A. W. Reinold, President, in the Chair.

The following communications were made:—

"On the Photometry of Colours." By Capt. W. de W. Abney.

"Note on Continuous Current Transformers." By Prof. S. P. Thompson.

"On an Optical Model." By Prof. A. W. Rücker.

"On a new Barometer." By Mr. T. H. Blakesley.

"On the Existence of an Undulatory Movement accompanying the Electric Spark." By Dr. Ernest H. Cook.

November 10th, 1888.

Prof. A. W. Reinold, President, in the Chair.

The following communications were made:—

"On the Calculation of the Coefficient of Mutual Induction of a Circle and a Coaxal Helix." By Prof. J. V. Jones.

"On the Upper Limit of Refraction in Selenium and Bromine." By the Rev. T. Pelham Dale.

"Experiments on Glass in Polarized Light." By Prof. S. P. Thompson.

"On a New Form of Standard Resistance-Coil." By Dr. J. A. Fleming.

November 24th, 1888.

Prof. A. W. Reinold, President, in the Chair.

The following were elected Members of the Society:—

Rev. Thomas Pelham Dale and Dr. Robert M. Walmsley.

The following communications were made:—

"On the Measurement of the Luminosity of Coloured Surfaces." By Capt. W. de W. Abney.

"On the Suppressed Dimensions of Physical Quantities." By Prof. A. W. Rücker.

December 8th, 1888.

Prof. A. W. Reinold, President, in the Chair.

The following were elected Members of the Society:—

Mr. A. P. Trotter and Dr. A. Gamgee, F.R.S.

The following communications were made:—

"Note on a Modification of the ordinary Methods of Determining Electro-Magnetic Capacity." By Dr. J. W. W. Waghorn.

"On some Facts connected with the Systems of Scientific Units of Measurement." By Mr. T. H. Blakesley.

"On some Improved Polarizing Apparatus for Microscopes." By Prof. S. P. Thompson.

January 26th, 1889.

Prof. F. Fuller, Member of Council, in the Chair.

The following communications were made:—

"Notes on Polarized Light: viz. On the Structure of Natural Diffraction-Gratings of Quartz; on Ahren's Modification of Delezenne's Polarizer; and on the Use of two Quarter-wave Plates in Combination with a Stationary Polarizer." By Prof. S. P. Thompson.

"Note on a Relation between Magnetization and Speed in a Dynamo-Machine." By Prof. S. P. Thompson.

"On the Divergence of Electromotive Forces from Thermochemical Data." By Prof. E. F. Herroun.

Annual General Meeting.

February 9th, 1889.

Prof. A. W. Reinold, President, in the Chair.

The following Report of the Council was read by the Chairman:—

The Council, in presenting their Annual Report, congratulates the Society on the success which their Meetings have maintained during the past year. The attendance at the Meetings has been from 25 to 40 Members and Visitors, and an ample supply of interesting papers has been brought forward. The Meetings have been somewhat shorter than in former years, as the Council have found that it was inconvenient to Members to have the Meetings prolonged much beyond half-past 4 o'clock.

The number of Members of this Society has increased to 434; but the Council, believing that if the advantages offered by the Society were more generally known a much greater increase might be reasonably expected, have had a fly-leaf in the following terms printed and circulated to the Members:—

"THE PHYSICAL SOCIETY OF LONDON.

"This Society was founded in 1874 for the purpose of receiving and discussing communications relating to Physics, and for the exhibition of apparatus for Physical research, and experiments illustrative of Physical phenomena, and for the publication of communications made to the Society, and other papers relating to Physics.

"The Society numbers between 300 and 400 Members.

"The Meetings are held on the 2nd and 4th Saturdays in each month from November to June inclusive, except that two meetings are omitted at Christmas and one at Easter.

"The time of meeting is 3 P.M.

"The place of meeting is the Physical Lecture-room at the Science Schools, South Kensington. This room is admirably suited for the exhibition of experiments, and the use of apparatus in the large collection belonging to the Science Schools is accorded to those who desire to exhibit experiments at the Society's Meetings; assistance is also rendered by the Demonstrator and his Staff in the preparation of such experiments.

"Every paper communicated to the Society is now published either in full or in abstract. If published in full it is published in the first instance in the 'Philosophical Magazine,' as communicated by this Society, and 100 copies of the paper are sent to the author. Papers which have thus appeared in the 'Philosophical Magazine' are afterwards reprinted in the Proceedings of this Society. Papers which the Council desire not to publish in full are published in abstract in the Proceedings of the Society.

"The Proceedings are as a rule published quarterly, and are issued gratis to Members.

"Besides the Proceedings, the Society from time to time issues other publications which are also supplied gratis to Members at the time of issue, and which may afterwards be purchased by Members at lower terms than by the public.

"The works of Wheatstone and of Joule have been thus published by the Society, and a series of translations of foreign papers has been commenced.

"The Library belonging to the Society is kept in a room adjoining the place of meeting. Application for leave to take out books should be made to the Librarian.

"The Entrance Fee is £1, and the Annual Subscription is also £1. Annual Subscriptions may be compounded for by the payment of £10 in one sum.

"Letters for the Officers of the Physical Society can be addressed to the Physical Laboratory, South Kensington, S.W.

"Members having friends outside the Society who are interested in the study of Physics are invited to draw their attention to the advantages which the Society offers.

"Additional copies of this fly-leaf can be obtained by application to the Secretaries."

In the past year the Society has issued to its Members the first part of its new undertaking, the Translation of important Physical Memoirs. This part contains five important papers by Prof. von Helmholtz on the Application of the Principles of Thermodynamics to Chemical Action and Electrolysis.

It was hoped that the translation of Puluj's memoir on Radiant Electrode-Matter would have also been issued; but for a more complete treatment of this subject it has been found desirable to include two additional papers by Hittorf on the Conduction of Electricity in Gases. The translation of them is well forward, and the whole part, it may be expected, will be in the hands of the Members early in the present session.

Prof. Threlfall, of the University of Sydney, having offered to the Society the translation by himself and Mr. Adare of Van der Waals' important memoir on the Continuity of the Gaseous and Liquid States of Matter, the Council has accepted the proposal with thanks, and has decided to undertake the publication of this translation.

A Committee, with the valuable help of Mr. H. H. Cunynghame, has settled what portions of Volta's work are to be undertaken, and the translation is being proceeded with. It must, however, be stated that the production of the above memoir will quite absorb whatever of the funds of the Society are available for this purpose in the present year.

The Council have observed with satisfaction the admirable manner in which the Meetings of the Society have been reported in the weekly periodicals, and they have directed that the reports of the General Meetings, as furnished by the Society's reporter, shall be published in the 'Proceedings,' in so far as the discussions are concerned, and that when a paper for any reason is not published *in extenso*, the abstract furnished by the reporter shall be published in the 'Proceedings.'

The following additions have been made to the Library of the Society:—

Newspapers:—

- Nature.
- The Electrical Review.
- Engineering.
- The Electrician.
- The Electrical Engineer.
- Scientific News.
- Electrical Plant.
- Ingeniero y Ferretero Español.
- Crónica Cientifica.

Magazines.

- The Philosophical Magazine.
- Beiblätter der Physik und Chemie.
- Annalen der Physik und Chemie.
- Journal de Physique.

Journals &c. of Societies:—

- Royal Society, Proceedings.
- Society of Arts, Journal.
- Society of Telegraph Engineers, Journal.
- Mechanical Engineers, Proceedings.
- Royal Meteorological Society, Quarterly Journal.
- Cambridge Philosophical Society, Proceedings.
- Middlesex Natural History Society, Transactions.
- Philosophical Society of Glasgow, Proceedings.
- Manchester Philosophical and Literary Society, Proceedings.
- Royal Dublin Society, Proceedings and Transactions.
- Royal Society of New South Wales, Journal.
- Canadian Institute, Proceedings.
- Academy of Natural Sciences, Philadelphia, Proceedings.
- Physical and Chemical Society of Russia, Journal.

Physical Society of Berlin, Proceedings.
Société Française de Physique, Séances.
College of Science of Japan, Journal.
Seismological Society of Japan, Transactions.
Atti R. Accad. Sci. Torino.

Books and Papers received :—

University College, London, Calendar.
Glasgow University, Calendar.
Imperial University of Japan, Calendar.
Geological Society, Report.
Observations, Hongkong.
Magnetic and Meteorological Observations, Bombay.
Institute of Civil Engineers, List of Members.
A Practicable Decimal System.
Four papers on Differential Equations, by Asutosh Mukhopadhyay.
University Studies, Nebraska.
Catalogue of Additions to Radcliffe Library, Oxford.
Life of Sir William Siemens.
Norwegian North-Sea Expeditions.
Hadfield on Manganese Steel.
Catalogue of Canadian Plants (Endogens).
List of Publications of Geological and Natural History Survey of Canada.
Prace Matematyezno-Fizyezne. Vol. I.
Stonyhurst College Observations.
Yorkshire College, Leeds, 14th Report.
Kew Observatory Report.
The Planets and The Cause of Light (Carruthers).
Sachregister Annalen, 1824–1877.
La Puissance Motrice du Feu (Carnot), given by Rev. A. Freeman.
Separatabdruck, by Dr. H. Bunte.
La Théorie Cinétique des Phénomènes Capillaires.

The set of the 'Philosophical Magazine' in the Society's Library has been completed by the purchase of the volumes for the four missing years, Messrs. Taylor and Francis having sold them to the Society at the lowest trade price, and having promised to present future numbers free of cost.

The Council have also received promises from the publishers of

'Nature,' the 'Electrician,' the 'Electrical Engineer,' and 'Engineering,' to send their future numbers free of cost.

So far as the Council are aware no death has occurred among the Ordinary Members of the Society; but the Council regret to have to announce the death of a distinguished Honorary Member, Prof. Clausius; and they nominate in his place Prof. Robert Wilhelm Bunsen, of Heidelberg, whose name is a household word in every Physical and Chemical Laboratory.

Rudolf Julius Emanuel Clausius was born January 2, 1822, at Cöslin, in Pomerania. About the year 1840 he entered the University of Berlin, in which he afterwards became a "Privat-Docent." After holding for some time the post of Instructor in Physics in the Artillery School at Berlin, he went in 1855 to Zurich, where he was Professor of Physics first in the Polytechnic and afterwards in the University. In 1867 he migrated to Wurtzburg; and in 1869 he was appointed Professor of Physics in the University of Bonn. This post he retained till his death in 1888.

His first paper, in Poggendorff's 'Annalen,' was entitled "Ueber die Lichtzerstreuung in der Atmosphäre und über die Intensität des durch die Atmosphäre reflectirten Sonnenlichts" (Pogg. *Ann.* vol. lxxii. p. 294), and he returned to the same or kindred subjects several times afterwards.

He will, however, be chiefly famous as one of the founders of the modern theory of Thermodynamics, in the establishment of which he played a part of the first importance.

It would be scarcely possible to give a detailed account of his work without entering on a somewhat extended history of the development of the Theory of Heat in the middle of the present century, and on a discussion as to questions of priority which have been somewhat hotly debated, and which would hardly be in place in a brief obituary notice. It is, however, pleasant to remember that in the last edition of his 'Theory of Heat,' published in 1887, Clausius expressed himself as fully satisfied with the treatment he had received at the hands of Sir W. Thomson and Maxwell, our countrymen who have done most to develop the subject in the same direction as that in which he himself worked.

In addition to his work on the 'Second Law of Thermodynamics' and on the 'Theory of Gas,' Clausius introduced the use of the Virial, a function of great generality, which has found an important application in the establishment of relations between the mean energy of molecular motions and intermolecular forces. In his work on the 'Anwendung der mechanischen Wärmetheorie zu Grunde liegenden Principien auf die Electricität,' he established an interesting equation, by which the ratio of the volume of the molecules to the total apparent volume of a dielectric is calculated from a knowledge of its inductive capacity. The results are in many cases in close accord with those deduced from the corresponding formula obtained from optical considerations by H. A. and L. Lorentz, and in the case of the gases are of the same order as the values given by the theory of Van der Waals.

Apart from his scientific work, the life of Clausius appears to have been devoid of stirring incident. Though his merits were recognized early, promotion—as the above record shows—did not come very quickly.

His 'Theory of Heat' has twice been translated into English, first by Dr. Hirst and afterwards by Mr. W. R. Browne. He was elected an Honorary Member of the Physical Society on February 14, 1880.

The adoption of the Report was proposed from the Chair, and carried unanimously.

The Society then proceeded to ballot for the Honorary Member nominated by the Council, and Professor Robert Wilhelm Bunsen was unanimously elected an Honorary Member of this Society.

The election of Officers and other Members of Council then took place, and the new Council was constituted as follows:—

President.—Prof. A. W. REINOLD, M.A., F.R.S.

Vice-Presidents who have filled the office of President.—Dr. J. H. GLADSTONE, F.R.S.; Prof. G. C. FOSTER, F.R.S.; Prof. W. G. ADAMS, M.A., F.R.S.; Sir WILLIAM THOMSON, D.C.L., LL.D., F.R.S.; Prof. R. B. CLIFTON, M.A., F.R.S.

Vice-Presidents.—Dr. E. ATKINSON; Prof. W. E. AYRTON, F.R.S.; SHELFORD BIDWELL, M.A., LL.B., F.R.S.; The Right Hon. Lord RAYLEIGH, M.A., D.C.L., Sec.R.S.

Secretaries.—WALTER BAILY, M.A.; Prof. J. PERRY, D.Sc., F.R.S.

Treasurer.—Prof. A. W. RÜCKER, M.A., F.R.S.

Demonstrator and Librarian.—C. V. BOYS, F.R.S.

Other Members of Council.—Hon. R. ABERCROMBY; T. H. BLAKESLEY, M.A., M.I.C.E.; W. H. COFFIN; CONRAD W. COOKE; Prof. O. LODGE, D.Sc., F.R.S.; Prof. W. RAMSAY, Ph.D., F.R.S.; W. N. SHAW, M.A.; Prof. S. P. THOMPSON, D.Sc.; H. TOMLINSON, B.A.; G. M. WHIPPLE, B.Sc.

Votes of thanks were passed to the Lords Committee of the Council on Education; to the OFFICERS; and to the AUDITORS.

The Treasurer in Account with the Physical Society, from January 1st, 1888, to December 31st, 1888.

Dr.	£	s.	d.	£	s.	d.
Balance in Bank, January 1, 1888				128	19	3
Entrance-Fees	10	0	0			
Subscription for 1881	1	0	0			
Subscriptions for 1882	1	0	0			
" 1883	1	0	0			
" 1884	1	0	0			
" 1885	1	0	0			
" 1886	2	0	0			
" 1887	5	0	0			
" 1888	117	0	0			
" 1889	4	0	0			
Excess Subscriptions	0	3	0			
Six Life Subscriptions	60	0	0			
				203	3	0
One year's Dividend on £400 Furness 4 per cent. Debenture Stock, less Income Tax	15	11	0			
One year's Dividend on £575 Midland 4 per cent. Preference Stock, less Income Tax	22	7	1			
One year's Dividend on £200 Lancaster Corporation Stock, less Income Tax	7	15	6			
One year's Dividend on £200 Metropolitan Board of Works Stock, less Income Tax	6	16	4			
				52	9	11
Received from Taylor and Francis:—						
Sales in 1888				33	1	6
Balance due to Treasurer				5	4	0
				£422	17	8

Cr.	£	s.	d.	£	s.	d.
Balance due to Treasurer				3	12	3
Excess Subscriptions returned				0	3	0
Bank charges				0	1	6
Taylor and Francis:—						
Proceedings, vol. ix. (parts 1–3)	103	8	0			
Circulars, postage and addressing	12	2	6			
Members' separate copies	21	12	6			
Miscellaneous printing	17	15	1			
Physical Memoirs, vol. i. part 1	54	4	6			
Books	5	12	0			
				214	14	7
Williams and Norgate				13	1	0
Gyde, for Binding				3	4	0
Attendance at Meetings and incidental expenses				7	1	11
Reporting				27	12	6
Brown, for Receipt-book &c.				0	10	9
Petty Cash:—						
Mr. Baily				1	18	0
Balance in Bank				150	18	2
				£422	17	8

Audited and found correct,

RICHARD INWARDS,
GEORGE M. MINCHIN, } *Auditors.*

London January 16th, 1889.

PROPERTY ACCOUNT OF THE PHYSICAL SOCIETY, December 31, 1888.

Assets.	£	s.	d.
Balance in Bank	150	18	2
Subscriptions due	145	0	0
£400 Furness 4 per cent. Debenture Stock	456	0	0
£575 Midland 4 per cent. Preference Stock	650	0	0
£200 Metropolitan Board of Works Stock	217	0	0
£200 Lancaster Corporation Stock	224	0	0
£254⅜ New South Wales 3½ per cent. Stock	249	7	5
Interest due (estimated dividend on N. S. Wales 3½ per cent.)	8	17	11
	£2101	3	6

Liabilities.	£	s.	d.
Balance due to Treasurer	5	4	0
Bills outstanding:—			
Taylor and Francis (one Part of Proceedings &c.)	54	13	8
Petty Cash	1	10	4
Subscriptions anticipated	4	0	0
Balance	2035	15	6
	£2101	3	6

We have examined the above Account, and also the Securities at the Bank, and find the same to be correct.

London, January 16th, 1889.

RICHARD INWARDS,
GEORGE M. MINCHIN, } *Auditors.*

PROCEEDINGS

AT THE

MEETINGS OF THE PHYSICAL SOCIETY

OF LONDON.

SESSION 1889-90.

February 9th, 1889.

Prof. A. W. Reinold, President, in the Chair.

The following were elected Members of the Society:—

Mr. James Wimshurst and Mr. Percy R. Sellon.

The following communication was made:—

"On Physico-Geometrical Models." By Prof. A. S. Herschel, F.R.S.

February 23rd, 1889.

Prof. A. W. Reinold, President, in the Chair.

Mr. Martin H. Kilgour was elected a Member of the Society.

The following communications were made:—

"On Electrical Measurement." By Prof. W. E. Ayrton, F.R.S., and Prof. J. Perry, F.R.S.

"Note on the Measurement of Resistance." By Dr. J. W. Waghorn.

"On the Dimensions of Electromagnetic Units." By Prof. G. F. FITZGERALD, F.R.S.

"Notes on Polarized Light." By Prof. S. P. THOMPSON.

March 9th, 1889.

Prof. A. W. REINOLD, President, in the Chair.

The following were elected Members of the Society:—

Prof. R. THRELFALL and Prof. A. S. HERSCHEL, F.R.S.

The following communication was made:—

"On the Rotation of the Plane of Polarized Light by the Discharge of a Leyden Jar." By Dr. OLIVER LODGE, F.R.S.

March 23rd, 1889.

Prof. A. W. REINOLD, President, in the Chair.

The following were elected Members of the Society:—

Mr. J. W. GRAHAM, Mr. C. CAPITO, Mr. D. G. FITZGERALD, Sir D. GALTON, Mr. W. H. MASSEY, Mr. R. E. CROMPTON, Mr. A. COCKBURN, Sir J. DOUGLASS, Mr. R. W. PAUL, Mr. E. MOYNIHAN, Mr. C. H. W. BIGGS, Mr. C. G. GÜMPEL, Mr. C. E. SPAGNOLETTI, and Mr. F. G. MUDFORD.

The following communications were made:—

"On the Use of Lissajous' Figures for determining Speed of Rotation; and on the Direct Determination of the Vibration Period of a Reed or Tuning-Fork by means of a Morse Receiver." By Prof. J. V. JONES.

"On the Clark Cell as a source of Standard Currents, and its application to the Calibration of Sensitive Long-Range Galvanometers." By Prof. R. THRELFALL and Mr. A. POLLOCK.

"On the Measurement of High Specific Resistances, and on the Measurements of the Resistance of Imperfectly Purified Sulphur." By Prof. R. THRELFALL.

April 13th, 1889.

Prof. A. W. **Reinold**, President, in the Chair.

The following were elected Members of the Society:—

Mr. P. W. **Willans**, Mr. J. E. **Dowson**, Mr. **Charles Priest**, Mr. C. A. **O'Keefe**, Mr. W. M. **Mordey**, and Mr. **Alexander Siemens**.

The following communications were made:—

"A Lecture-Experiment illustrating the Effect of Heat on the Magnetic Susceptibility of Nickel;"

"An Experiment showing the Effect of Light on Magnetism." Both by Mr. **Shelford Bidwell**, F.R.S.

"On the Dark Flash as seen in some Lightning Photographs." By Mr. G. M. **Whipple**.

"On Quartz as an Insulator." By Mr. C. V. **Boys**, F.R.S.

"On a Refraction-Goniometer." By Mr. A. P. **Trotter**.

"Apparatus to illustrate Crystal Forms." By Dr. R. J. **Anderson**.

May 11th, 1889.

Prof. A. W. **Reinold**, President, in the Chair.

The following were elected Members of the Society:—

Mr. W. **Robinson**, Prof. V. B. **Lewes**, and Mr. A. **Clegg**.

The following communications were made:—

"On an Electrostatic Field produced by varying Magnetic Induction." By Dr. O. **Lodge**, F.R.S.

"On Electric Radiation, and its Concentration by Lenses." By Dr. O. **Lodge**, F.R.S., and Dr. J. **Howard**.

May 25th, 1889.

Prof. A. W. **Reinold**, President, in the Chair.

Mr. T. A. **Lawson** was elected a Member of the Society.

The following communications were made:—

"On a Relation existing between the Density and Refraction of Gaseous Elements, and also of some of their Compounds." By the Rev. T. **Pelham Dale**, M.A.

"On a Water-Spray Influence-Machine." By Mr. George Fuller.

"Notes on Polarized Light." By Prof. S. P. Thompson.

"On the Molecular Weight of Caoutchouc and other Colloid Bodies." By Dr. J. H. Gladstone, F.R.S., and Dr. Walter Hibbert.

"A Note on Mr. Pelham Dale's paper." By Prof. A. W. Rücker, F.R.S.

June 8th, 1889.

Prof. Ayrton, Vice-President, in the Chair.

The following were elected Members of the Society:—

Mr. Ludwig Mond and Mr. John R. Brittle.

The following communications were made:—

"On Intermittent Lightning Flashes." By Dr. H. H. Hoffert.

"On the Methods of suppressing Sparking in Electro-Magnetic Circuits." By Prof. S. P. Thompson.

"On a Shunt-Transformer." By Mr. E. W. Smith.

"Notes on Geometrical Optics." By Prof. S. P. Thompson.

"On the use of the Bi-Quartz in determining the Position of the Plane of Polarization." By Mr. A. W. Ward.

June 22nd, 1889.

Prof. A. W. Reinold, President, in the Chair.

The following were elected Members of the Society:—

Mr. A. W. Ward and Major Cardew.

The following communications were made:—

"On some Photographs of Lightning, and of *Black* Electric Sparks." By Mr. A. W. Clayden.

"Researches on the Electrical Resistance of Bismuth." By Edmond van Aubel.

"On the Expansion with Rise of Temperature of Wires under Pulling Stress." By Mr. J. T. Bottomley, F.R.S.

November 1st, 1889.

Prof. **Reinold**, President, in the Chair.

Mr. T. A. **Garrett** was elected a Member of the Society.

The following communications were made:—

"On a new Electric Radiation-Meter;"

"On a Method of driving Tuning-Forks Electrically." Both by W. G. **Gregory**, M.A.

"On a Physical Basis for the Theory of Errors." By Mr. C. V. **Burton**.

"On the Behaviour of Twisted Strips." By Prof. **Perry**, F.R.S.

"On Electrifications due to the Contact of Gases and Liquids." By Mr. J. **Enright**.

November 15th, 1889.

Prof. **Reinold**, President, in the Chair.

The following were elected Members of the Society:—

Prof. J. **Milne**, F.R.S., and Lieut. G. F. **Eyre**, R.N.

The following communications were made:—

"On Electrifications due to the Contact of Gases and Liquids." By Mr. J. **Enright**.

"On the Effect of repeated Heating and Cooling upon the Electrical Resistance of Iron." By Mr. **Herbert Tomlinson**, F.R.S.

December 6th, 1889.

Prof. **Reinold**, President, in the Chair.

Mr. J. W. **Swan** was elected a Member of the Society.

The following communications were made:—

"On the Electrification of a Steam-Jet." By **Shelford Bidwell**, M.A., F.R.S.

"Notes on Geometrical Optics." By Prof. S. P. **Thompson**.

"On the Behaviour of Steel under Mechanical Stress." By Mr. C. A. **Carus-Wilson**.

January 17th, 1890.

Prof. Adams, Vice-President, in the Chair.

The following communications were made :—

"On Electric Splashes." By Prof. S. P. Thompson.

"On Galvanometers." By Prof. Ayrton, F.R.S., Mr. T. Mather, and W. E. Sumpner, D.Sc.

Annual General Meeting.

February 7th, 1890.

Prof. A. W. Reinold, President, in the Chair.

The following Report of the Council was read by the Chairman :—

The Council have again the satisfaction of recording an increase in the number of Members, which now exceeds 360. It may be worthy of note that among our Members are about 80 Fellows of the Royal Society.

The Meeting of the Society on Saturday afternoons has long been felt by Members to be inconvenient. Some years ago the Council issued a circular asking for the opinions of Members on this question, and it was ascertained that the Society was nearly equally divided in opinion, the majority in favour of a change being so small that the Council did not feel themselves justified in disturbing the then existing arrangement. In the past year the question was reconsidered, and after carefully taking into account the times of meeting of other Societies whose work is closely connected with our own, and whose Meetings many of our own Members are accustomed to attend, the Council came to the conclusion that the afternoons of the 1st and 3rd Fridays in each month was the only time to which our meetings could be transferred without interfering with the attendance of our Members at the meetings of other Societies. The Council issued a circular asking Members whether they were in favour of, or against, or indifferent to the suggested change; and they received a large number of replies, giving a very strong majority in favour of the change—the numbers being in favour 129, indifferent 54, against 30. The Council accordingly decided to change the time of meeting to the 1st and 3rd Fridays, at 5 P.M. The change came into effect last November, and is found more convenient than the Saturday afternoons to the Members who attend the meetings, although it has not produced a satisfactory increase in the numbers of those who attend.

During the present Session the second part of the Translations of Foreign Scientific Memoirs has been issued to the Members. This embraces four Memoirs by Prof. Hittorf on the Conduction of Electricity in Gases, and a Memoir by Dr. Puluj on Radiant Electrode Matter, the whole containing 331 pages, with 68 woodcuts and three lithographic plates.

Prof. Threlfall and Mr. Adare, having completed the translation of Van der Waals' memoir on the Continuity of the Liquid and Gaseous States of Matter, referred to in last year's report, the whole of this is now in type. In order to save time, the proofs read for press have been forwarded to Sydney, and as they are returned the work will be printed off, and the Committee confidently expect that it will be issued to the Members during the early part of the present Session.

No further progress has been made with Volta's works during the year, an important public engagement having prevented Mr. Cunynghame from giving any active help. This engagement being now at an end, it may be anticipated that the Society may look for this help, and that the remainder of the work will be ready in the course of the present year. The publication, however, of Van der Waals' memoir is as much as can be looked for at present.

The Council recommend the election of Prof. Hittorf, of Münster, as an Honorary Member of the Society.

The Council have the pleasure of announcing that the following valuable collection of books relating to Electricity has been offered by the Council of the Royal Astronomical Society, and will form a welcome addition to our Library:—

Natural Philosophy.

ADAMS, G. Essay on Electricity. 8vo. 1784.

ALDINI, J. Account of Improvements in Galvanism. 4to. 1803.

BENNET, A. New Experiments on Electricity. 8vo. Derby, 1789.

BROOK, A. Experiments and Remarks on Electricity (&c.). 4to. Norwich, 1789.

CAVALLO, T. Complete Treatise on Electricity. 4th edit. 3 vols. 8vo. 1795.

CAVALLO, T. Treatise on Magnetism. 2nd edit. 8vo. 1795.

CAVALLO, T. Elements of Natural or Experimental Philosophy. 4 vols. 8vo. 1803.

FERGUSON, J. Introduction to Electricity. 8vo. (2 copies.) 1770.

FRANKLIN, B. Experiments and Observations on Electricity. 4to. 1769.

FRANKLIN, B. New Experiments and Observations on Electricity. 3 parts, in 1 vol. 4to. 1760–65.

[*Bound with above Vol.*]

SYMMER (R.). Experiments and Observations on Electricity. 4to. 1760.

PRIESTLEY, J. Account of Henly's Electrometer. 4to. 1773.

HENLY, W. Experiments concerning Lightning Conductors. 1774.

HENLY, W. New Experiments in Electricity. 4to. 1774.

PRIESTLEY, J. Familiar Introduction to Electricity. 1768.

FRANKLIN, B. Philosophical and Miscellaneous Papers. 8vo. 1787.

FRANKLIN, B. Works. 2nd edit. 3 vols. 8vo. [n. d.]

HOLMES. Magneto-Electric Light in Lighthouses. 8vo. [1862.]

LORIMER, J. Essay on Magnetism. 4to. 1795.

MAHON, Lord. Principles of Electricity. 4to. 1779.

MUSSCHENBROEK, P. VAN. Elements of Natural Philosophy. Colson. 2 vols. 8vo. 1744.

PRIESTLEY, J. History and Present State of Electricity. 4to. 1767.

PRIESTLEY, J. Familiar Introduction to the Study of Electricity. 4to. 1768.

SINGER, G. J. Elements of Electricity. 8vo. 1814.

WATSON, W. Sequel to Experiments and Observations on Electricity. 8vo. 1746.

WILKINSON, C. H. Elements of Galvanism. 2 vols. 8vo. 1804.

The following Journals and Books have been received by the Physical Society:—

Newspapers and Magazines:—

Nature.
The Electrical Review.
Engineering.
The Electrician.
The Electrical Engineer.
Electrical Plant.
Ingeniero y Ferretero Español.
Crónica Cientifica.
The Philosophical Magazine.
Beiblätter der Physik und Chemie.
Annalen der Physik und Chemie.
Journal de Physique.

Journals &c. of Societies, British:—

Royal Society, Proceedings.
Society of Arts, Journal.
Institution of Electrical Engineers, Journal.
Mechanical Engineers, Proceedings.
Royal Meteorological Society, Quarterly Journal.
Royal Dublin Society, Proceedings and Transactions.
Middlesex Natural History and Science Society, Transactions.
Philosophical Society of Glasgow, Proceedings.
Philosophical Society of Cambridge, Proceedings and Transactions.
Literary and Philosophical Society of Manchester, Memoirs and Proceedings.
Royal Institution, Proceedings.
Geological Society of Glasgow, Transactions.
Birmingham Philosophical Society, Proceedings.

Journals of Societies, Colonial and Foreign:—

Royal Society of New South Wales, Journal and Proceedings.
Canadian Institute, Proceedings.
New Zealand Institute, Transactions.
Magnetic and Meteorological Observations, Bombay.
Academy of Natural Sciences, Philadelphia, Proceedings.
Physical and Chemical Society of Russia, Journal.
Physical Society of Berlin, Verhandlungen.
Physical Society of France, Séances. Mémoires, Tome iv.
College of Science of Japan, Journal.
Seismological Society of Japan, Transactions.
R. Accad. Torino, Atti and Bulletino.
Cincinnati Society of Natural History, Journal.
Bulletin International de l'Académie des Sciences de Cracovie, Comptes-Rendus.
St. Louis Academy of Science, Transactions.
New York Academy of Science, Transactions.
Ohio Meteorological Bureau, Report.
Bulletino della Societa Ital. di Elettricita.

Books and Papers:—

Calendars of Glasgow University, City of London College, and University of London.
Institution of Civil Engineers, Charter and List of Members.

Society of Chemical Industry, Address of President.	
Sur les Propriétés Physiques de la Couche Superficielle libre d'un Liquide. Sur un genre particulier d'expérience Capillaire.	G. Van de Mensbrugghe.
Winding of Dynamo Fields. Report of Missouri Rainfall. Output of Non-Condensing Steam-Engines.	Francis E. Nipher.
Sur les Phénomènes Actino-Eléctriques, by M. A. Stoltetow.	
Contributions to Canadian Palæontology, Vol. I.	
Prace Matematyczno-Fizyczne, Tome II. pt. 1.	

The Council regret to have to record the loss by death of three well-known Members of the Society—Dr. James Prescott Joule, Mr. Warren De La Rue, and the Rev. Stephen Joseph Perry.

JAMES PRESCOTT JOULE died at Sale, near Manchester, on Friday night, October 11th, 1889, after many years of ill-health. It is admitted by those most competent to judge that his name must be classed alongside of the great original workers in science. Many who have never heard of the name of Dr. Joule have heard of the 'Mechanical Equivalent of Heat,' a phrase, it is to be feared, that to the majority of even intelligent people has little meaning. It is mainly on the determination of this 'Mechanical Equivalent' that the late Dr. Joule's title to fame rests. Joule was born at Salford, Manchester, on Christmas Eve, 1818, so that he has died at a ripe old age. He was so delicate that he could not be sent to school, and so was privately educated at home. Both his father and grandfather were brewers, and young Joule and his brother also entered the business, though James soon became so absorbed in scientific investigations that he practically left the brewery to others. Soon after entering the brewery, about the age of 15, he and his brother were sent by their father to the great chemist Dalton, the propounder of the atomic theory, in order to learn chemistry. One can easily fancy the influence that contact with a mind like Dalton's would have on a youth like Joule. He seems to have been at once caught with the enthusiasm for scientific research, and soon, with the rudest apparatus, began to make experiments of his own, mainly on the chemical constitution of gases and on the relations between chemical action and electricity. One of his first published papers was on a new Electro-Magnetic Engine, which he described at the age of 19. The amount and variety of his work since then may be inferred from the fact that in the Royal Society List there are the titles of 97 papers, besides over 20 other important papers on researches undertaken by him in conjunction with Sir William Thomson, Sir Lyon Playfair, and Dr. Scoresby. All his papers, revised by himself, have been published by the Physical Society.

In conducting his many experiments in Electromagnetism he had met with "the difficulty, if not impossibility, of understanding experiments and comparing them with one another, which arises in general from incomplete descriptions of apparatus, and from the arbitrary and vague numbers which are used to characterize electric currents . . . . I have therefore determined for my own part to abandon my old quantity numbers and to express my results on the basis of a unit which shall be at once scientific and convenient." He set himself to devise an accurate method of measuring a unit-quantity of electricity, which he took to be the quantity required to decompose nine grains of water. He went on to devise many improvements in galvanometers and to carry out a vast variety of ingenious experiments on the various aspects of electricity, and was thus led in 1841 and 1842 to lay the foundation of a new department of physical science—Thermodynamics, in which so much good work has been done since. Meantime he had been speculating on the conversion of chemical energy into heat; and in 1840, 1841, and 1842 he gave accounts of a number of experiments, all leading up to the great determination of the Mechanical Equivalent of Heat. This value in its earliest form was first announced at the Cork Meeting of the British Association in 1843, in a paper on "The Calorific Effects of Magneto-Electricity, and on the Mechanical Value of Heat." In it, among other things, he said:—"I have lately found experimentally that heat is evolved by the passage of water through narrow tubes. . . . I obtained one degree of heat per pound of water from a mechanical force capable of raising about 770 lb. to the height of one foot. . . . I shall lose no time in repeating and extending these experiments, being satisfied that the grand agents of nature are, by the Creator's fiat, indestructible, and that, whatever mechanical force is expended, an exact equivalent of heat is always obtained." The importance of the announcement it is impossible to overrate. Of course others at home and abroad had been working in the same direction, just as had been the case before Darwin announced his great discovery. But, though at first it was slowly and somewhat grudgingly recognized by his fellow-students, Joule's discovery made its way surely, and is now recognized as one of the ruling constants of science. Of course he continued to work at, and to perfect, his mechanical equivalent; and in a paper to the Royal Society in 1878 he gave the results of a fresh determination, according to which the quantity of work required to be expended in order to raise the temperature of 1 lb. of water, weighed in vacuum, from 60° to 61° Fahrenheit, is 772·55 foot-pounds of work, at the sea-level, and in the latitude of Greenwich. Joule's great experiment has been repeated by many hands and in many forms and on great scales, but nothing has shaken the truth of the discovery.

The scientific bearings of Joule's great discovery are of the widest and profoundest character; and he is entitled to be placed among the greatest of original workers in pure science. And the practical results which have accrued from his theory have been of the most useful and beneficent kind. They have enabled our engineers to improve their machinery along lines of precision not otherwise attainable; while the amount saved in the application of heat must have amounted to millions. Joule continued his scientific investigations till very near the end, and, varied and important as they were, they cannot be entered into here. He lived the most retired life, partly from weak health and partly from his native modesty; and so it happens that one of the greatest of Darwin's contemporaries was hardly known to the outside world. He was honoured, among those whose opinions alone he valued, at home and abroad. Gold Medals and other marks of appreciation were showered upon him, including those of the Royal Society and Society of Arts. In 1878 Her Majesty, through Lord Beaconsfield, conferred upon him a pension of £200 a year; and few pensions were ever better deserved.

WARREN DE LA RUE, D.C.L., F.R.S., Ph.D., who died on Good Friday, 19th April, 1889, was the son of the late Mr. Thomas De La Rue, and was born in Guernsey in the year 1815. He was educated in Paris, and succeeded his father as head of the Firm of Thomas De La Rue and Co., from which he retired in 1880. He acted as juror and reporter in the Department of Class XXIX. in the Great Exhibition of 1851, was a juror in Class X. of the Paris Exhibition of 1855, and presided over Section B Class XXVIII. of the Exhibition of 1862. In 1860 he went to Spain with the Himalaya Expedition, and was successful in obtaining a series of photographs of the total eclipse of the sun on July 18th. The results formed the subject of the Bakerian Lecture, delivered to the Royal Society in April 1862. In conjunction with Professor Balfour Stewart and Mr. B. Loewy he published 'Researches in Solar Physics,' founded on observations made at the Kew Observatory under his directions. He also took an active part in making the preparations for the photographic observation of the Transit of Venus in 1874. He established a private Observatory at Cranford, Middlesex; but it was dismantled in 1873, and the instruments presented to the University of Oxford, where they have been employed most successfully by Professor Pritchard in determining, by means of photography, the distance of 61 Cygni and other fixed stars. In 1874 he fitted up a private Physical Laboratory, where, employing a battery of 15,000 chloride-of-silver cells, he, in conjunction with Dr. Hugo Müller, carried on an elaborate series of researches on the electrical discharge. The results of the researches were communicated to the Royal Society and the Académie des Sciences, Paris. He acted for some time as Honorary Secretary of the Royal Astronomical Society, of which he was also President from 1864 to 1866. He was President of the Chemical Society from 1867 to 1869, and again in 1879-88. He also filled the offices of President of the London Institution and Secretary of the Royal Institution. He was a Corresponding Member of the French Académie des Sciences, for the Department of Astronomy; also of the Imperial Academy of Sciences, St. Petersburg, and other Foreign Societies. Three foreign orders were conferred upon him:—Commander of the Legion of Honour, Commander of the Order of St. Maurice and St. Lazare, and Knight of the Order of the Rose, Brazil. He was a Member of this Society almost from its commencement.

Stephen Joseph Perry was born in London on August 26, 1833. After studying at the Catholic College at Douay, he went to Rome to study philosophy, and entered the Order of the Jesuits at the age of twenty. A few years later he went through a special course of Mathematics in Paris; and in 1860 was appointed Professor at Stonyhurst College and Director of the Observatory, which positions he held till his death, with the exception of five years spent in studying theology at St. Beuno's College, North Wales, and in making a Magnetic Survey of France. He was ordained priest in 1866. On April 9, 1869, Father Perry was elected a Fellow of the Royal Astronomical Society, and on June 4, 1874, a Fellow of the Royal Society, and he was one of the original Members of the Physical Society. Among the other Societies with which he was connected as Member or Associate are the Royal Meteorological, the Brussels Academy of Sciences, and the Accademia dei Lincei.

Some idea of the esteem in which astronomers held their departed colleague may be inferred from the fact that on more than one occasion he was a Member of the Council of the Royal Astronomical Society, and was recently elected President of the Liverpool Astronomical Society. The reason for this esteem is not far to seek, for to the qualities of an able and enthusiastic astronomer Father Perry added a readiness to aid all who sought his counsel, and he commanded the respect of all by his unselfishness where astronomy was concerned. To many persons a trip on an astronomical expedition would be a pleasant holiday; to him it was a terrible trial, as he was a martyr to sea-sickness. Notwithstanding this, we find him in 1874 at Kerguelen, and in 1882 at Madagascar, in 1886 in the West Indies, and in 1887 in Russia. Only those who were his companions on these occasions are fully aware of his devotion to Science displayed in voluntarily undergoing so much suffering. His correspondence contains no reference to it; but Mr. Turner, of the Royal Observatory (who went to the West Indies in 1886), in an astronomical journal published this morning, says:—"Father Perry has been one of the most consistent and cheerful sufferers in the cause of Science. He was always a martyr to sea-sickness, but has been a member of more Astronomical Expeditions than any man now living."

Last year he went in charge of the Eclipse Expedition to the Salut Islands, where an attack of dysentery seized him at 3 o'clock on the morning of the eclipse. So rapidly weak did Father Perry become that he had to be assisted to the Observatory at the Salut Islands. Feeling uncertain as to the course his illness would take, Father Perry requested and received the last Sacrament from the resident priest before her Majesty's ship 'Comus' left the island for Demerara. Father Perry's worst fears, however, were realized, and before the vessel arrived at Demerara the indefatigable astronomer departed this life, on December 12th, 1889.

His solar work during the last ten years formed the subject of a lecture at the Royal Institution on May 24. It may be divided into two classes—drawings and spectroscopic observations. For the drawings an image of the sun $10\frac{1}{2}$ inches in diameter was projected on a sheet of drawing-paper affixed to a sketch-board carried by the telescope, and all markings on the sun traced. The drawing finished, the chromosphere and prominences were examined with the spectroscope. About 250 drawings were made every year from 1880.

The adoption of the Report was proposed, and carried unanimously.

The Society then elected Professor W. Hittorf an Honorary Member of the Society.

The election of Officers and other Members of Council then took place, and the new Council was constituted as follows:—

President.—Prof. W. E. AYRTON, F.R.S.

Vice-Presidents who have filled the office of President.—Dr. J. H. GLADSTONE, F.R.S.; Prof. G. C. FOSTER, F.R.S.; Prof. W. G. ADAMS, M.A., F.R.S.; Sir WILLIAM THOMSON, D.C.L., LL.D., F.R.S.; Prof. R. B. CLIFTON, M.A., F.R.S.

Vice-Presidents.—Dr. E. ATKINSON; WALTER BAILY, M.A.; SHELFORD BIDWELL, M.A., LL.B., F.R.S.; Prof. S. P. THOMPSON, D.Sc.

Secretaries.—Prof. J. PERRY, F.R.S.; T. H. BLAKESLEY, M.A., M.Inst.C.E.

Treasurer.—Prof. A. W. RÜCKER, M.A., F.R.S.

Demonstrator and Librarian.—C. VERNON BOYS, F.R.S.

Other Members of Council.—W. H. COFFIN; Sir JOHN CONROY, Bart., M.A.; CONRAD W. COOKE; Major-General E. R. FESTING, R.E., F.R.S.; Prof. J. V. JONES, M.A.; Prof. O. LODGE, D.Sc., F.R.S.; Prof. W. RAMSAY, Ph.D., F.R.S.; W. N. SHAW, M.A.; H. TOMLINSON, B.A., F.R.S.; G. M. WHIPPLE, D.Sc.

Votes of thanks were passed to the Lords Committee of the Council on Education; to the OFFICERS; and to the AUDITORS.

The Treasurer in Account with the Physical Society, from January 1st, 1889, to December 31st, 1889.

Dr.	£	s.	d.	£	s.	d.
Balance in Bank, January 1, 1889				150	18	2
Entrance-Fees	29	0	0			
Subscriptions for 1882	1	0	0			
" 1883	1	0	0			
" 1884	2	0	0			
" 1885	2	0	0			
" 1886	2	0	0			
" 1887	4	0	0			
" 1888	18	0	0			
" 1889	131	0	0			
" 1890	1	0	0			
Excess Subscriptions	1	15	0			
Eleven Life Subscriptions	110	0	0			
				302	15	0
One year's Dividend on £400 Furness 4 per cent. Debenture Stock, less Income Tax	15	12	0			
One year's Dividend on £575 Midland 4 per cent. Preference Stock, less Income Tax	22	8	6			
One year's Dividend on £200 Lancaster Corporation Stock, less Income Tax	7	16	0			
One year's Dividend on £200 Metropolitan Board of Works Stock, less Income Tax	6	16	8			
Two years' Dividends on £254⅜ New South Wales 3½ per cent. Stock	17	6	7			
				69	19	9
Received from Taylor and Francis :—						
Sales in 1889				20	14	11
Balance due to Treasurer				7	5	6
				£551	13	4

Cr.	£	s.	d.	£	s.	d
Balance due to Treasurer				5	4	0
Excess Subscriptions returned				1	15	0
Taylor and Francis :—						
Proceedings, vol. ix. (part 4) ; vol. x. (parts 1 & 2)	124	1	0			
Circulars, addressing and posting	12	10	6			
Members' separate copies	24	19	0			
Miscellaneous printing	28	0	4			
Physical Memoirs	101	13	6			
				291	4	4
Williams and Norgate				4	5	0
Haggard				13	11	6
Attendance at Meetings and incidental expenses				7	7	6
Reporting				14	1	8
Petty Cash :—						
Prof. Rücker				2	10	4
Balance in Bank				211	14	0
				£551	13	4

Examined and found correct,

January 30th, 1890.

GEORGE M. MINCHIN, } *Auditors.*
ALFRED H. FISON,

PROPERTY ACCOUNT OF THE PHYSICAL SOCIETY, December 31, 1889.

Assets.	£	s.	d.	Liabilities.	£	s.	d.
Balance in Bank	211	14	0	Balance due to Treasurer	7	5	6
Subscriptions due	157	0	0	Bills outstanding:—			
£400 Furness 4 per cent. Debenture Stock	456	0	0	Gyde	1	18	0
£575 Midland 4 per cent. Preference Stock	650	0	0	Subscriptions anticipated	1	0	0
£200 Metropolitan Board of Works Stock	217	0	0	Balance	2154	17	11
£200 Lancaster Corporation Stock	224	0	0				
£254¾ New South Wales 3½ per cent. Stock	249	7	5				
	£2165	1	5		£2165	1	5

Examined, and found correct.

GEORGE M. MINCHIN, } *Auditors.*
ALFRED H FISON, }

January 30th, 1890.